THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.
AND
PROF. E. T. WHITTAKER, Sc.D., F.R.S.

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The Mathematical Association.

THE Annual Meeting of the Mathematical Association was held at the London Day Training College.

On Thursday, January 5th, the evening lecture * was given by Prof. S. Brodetsky, who took *Gravitation* as his subject.

On Friday, January 6th, after the Report of the Council was received and the formal business was transacted, Prof. M. J. M. Hill delivered his Presidential Address. He took for its title *The Logical Eye and the Mathematical Eye: their Outlook on Euclid's Theory of Proportion.*

The President, who was suffering from blindness, gave from memory an able and vigorous exposition of a subject to which he has devoted years of thought. Those who were privileged to hear him were filled with sympathy and admiration at the fortitude with which he faced and the success with which he surmounted his physical disability.

Prof. A. Lodge then took the chair.

Dr. Sheppard discussed in a short paper * the meaning of a Partial Differential Coefficient.

Mr. W. Hope-Jones then dealt in characteristic style with the principles of Sound-Ranging.

In the afternoon, *The Organisation of School Mathematics* and *The Best Method of Examining School Mathematics*, were the subjects selected by Messrs. P. Bolton and A. W. Siddons respectively. In the ensuing discussions part was taken by Mr. A. J. Taylor, Miss Waters, Mr. Katz, Mr. C. H. Thompson and others.

A vote of thanks to the Chairman brought the proceedings to a close.

* Publication unavoidably postponed.

REPORT OF THE COUNCIL FOR THE YEAR 1927.

DURING the year 1927, 83 new members of the Association have been admitted. The number of members now on the Roll is 1163, and of these 8 are honorary members, 59 are life members by composition, 22 are life members under the old rule (now rescinded), and 1074 are ordinary members. The number of Associates is about 550.

The Council regret to have to record the deaths of Sir George Greenhill, F.R.S.; Mr. R. F. Davis; Mr. George Debayo Agbebi, of Lagos; Mr. H. A. L. Barclay, of Bexhill-on-Sea; Miss Kate Fleming of the Girls' High School, Skipton; and Mr. William Powell, of the Royal Grammar School, Lancaster. Sir George Greenhill was President of the Association for the years 1913 and 1914, and was always very regular in his attendance at the meetings of the Council. A memoir of Sir George will appear in an early number of the *Gazette*. Mr. R. F. Davis was Honorary Auditor of the Association from 1897 to 1916; a memoir was published in the issue of the *Gazette* for October.

The volume prepared in commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton, mention of which was made in the last report, was distributed to members early in the year. It was planned by Mr. Greenstreet, who also devoted himself unsparingly to the work of Editorship. The indebtedness of the Association to the many distinguished contributors is expressed, on behalf of the Council, in the Preface. With the permission of the Council, Messrs. G. Bell & Sons have issued an edition of the book for the general public, and copies in this form can still be obtained at 10s. 6d. each.

The Yorkshire Branch of the Association organised Newtonian celebrations at Grantham. On Friday, 18th March, the visitors were entertained at a Civic Reception by his Worship the Mayor of Grantham. On Saturday morning, 19th March, a meeting was held at the Old School. Dr. J. H. Jeans, Sec. R.S., was in the chair, and addresses were given by Sir J. J. Thomson, O.M., F.R.S., Sir Frank Dyson, F.R.S., and Professor L. J. Mordell, F.R.S. In the afternoon, Newton's birthplace (Woolsthorpe Manor House) was visited, and in the evening Sir J. J. Thomson presided at a Dinner. On Sunday morning, 20th March, the Bishop of Birmingham (Dr. E. W. Barnes, F.R.S.) preached at a special service at the Grantham Parish Church. A full account of these celebrations appeared in a Special Supplement of *Nature*, 26th March, 1927.

The Teaching Committees have hitherto held office for two years only. The Council now consider that it is desirable that this period should be extended from two years to four years, so that the existing Committees will remain in office until January 1930.

Professor M. J. M. Hill retires after this meeting from the office

of President, and the Council desire to express their most grateful thanks to him for the services which he has rendered to the Association. The Council feel that they are speaking for all members in expressing sympathetic admiration for the courage with which he has faced the increasing disabilities under which his work has been carried on during his two years of office.

The Council nominate Dr. W. F. Sheppard to be President of the Association for the year 1928, and Professor M. J. M. Hill to be a Vice-President.

The Council desire to express their thanks to Mr. H. K. Marsden and Professor W. M. Roberts, who now retire from the Council in accordance with the rules, for the valuable services they have rendered to the Association during their tenure of office as members of the Council since 1923 and 1921 respectively.

Members have made slightly less use of the Library this year than last. Mr. Beames has continued to render valuable service in despatching and acknowledging books. The most important accession was from the collection of Sir George Greenhill.

The Council desire again to acknowledge the indebtedness of the members of the Association to Mr. W. J. Greenstreet for the continuance of his admirable work as Editor of the *Mathematical Gazette*, and to express their thankfulness for his good recovery from the serious illness which overtook him during the year. The Association was very fortunate in that Professor E. H. Neville was able to carry on the editorial work in this emergency, and the cordial thanks of the Association are due to him for this service in addition to his work as Librarian.

The Election of Officers and Council was then proceeded with. Mr. W. F. Sheppard, Sc.D., LL.M., was elected President for the year 1928. Prof. M. J. M. Hill, F.R.S., retiring President, was elected a Vice-President. Mr. W. C. Fletcher, M.A., and Professor G. B. Jeffery, D.Sc., were elected Members of the Council in succession to Mr. H. K. Marsden and Professor W. M. Roberts. The other Officers and Members of the Council were re-elected.

GLEANINGS FAR AND NEAR.

486. We must endeavour to persuade those who are to be the principal men of the State to go and learn arithmetic, not as amateurs, but they must carry on the study until they see the nature of numbers with the mind only; nor again, like merchants or retailers, with a view to buying or selling, but for the sake of their military use, and of the soul herself; and because this will be the easiest way for her to pass from becoming to truth and being.—Plato, *Republic*, vii. 525. Jowett, p. 227 (1888).

487. . . . Keeping his mind so intent on the operation, as to have power to think of nothing else,—common-place infirmity of the greatest mathematicians! working with might and main at the demonstrations, and so wasting all their strength upon it, that they have none left in them to draw the corollary to do good with.—Sterne, *Tristram Shandy*, i. p. 109 (Dent) 1894.

THE LOGICAL EYE AND THE MATHEMATICAL EYE.

THEIR OUTLOOK ON EUCLID'S THEORY OF PROPORTION.

Presidential Address to the Mathematical Association, 1928.

By Professor M. J. M. Hill, F.R.S.

Introduction.

1. My address last year was devoted to the consideration of a few rules or statements made in teaching, with, as it seemed to me, insufficient explanation, in the hope that I might suggest some line of action, in a few particular cases, which would be helpful to members of our Association engaged in elementary teaching.

The greater part of that address was concerned with the *first* of Euclid's two great pronouncements, viz., the Postulate of Parallels, and to the suggestion of an alternative treatment, based on Wallis's Postulate of Similarity, and it was shown that Wallis's Postulate, invented 264 years ago to explain Euclid's Postulate of Parallels, sufficed also to explain his Fourth Postulate, viz. that all right angles are equal.

2. To-day, with a similar object in view, in the hope of assisting those members of the Association who are preparing mathematical pupils intending to enter a University, and those of you who are teaching students of mathematics during the first year of a University course, I take up the *second* of Euclid's two great pronouncements, viz. the famous Definition of Proportion, the Fifth Definition of the Fifth Book of the Elements, and the argument based thereon.

This definition, which is attributed by tradition to Eudoxus, the master of Plato, is, when it makes its first appearance in Euclid's text, no less startling than the Postulate of Parallels.

The Two Views.

3. The title I have given to this Address was suggested to me by a saying of De Morgan, which forecasts the treatment of the subject here adopted.

It is on record that he said once : *

"The two eyes of exact science are mathematics and logic; the mathematical sect puts out the logical eye, the logical sect puts out the mathematical eye, each believing it can see better with one eye than with two."

The distinction between the views obtainable from the two eyes may be stated thus :

"From the time of Aristotle downwards logic has always meant the art of making correct deductions from the principles employed. It looks at the verbal treatment of the terms of an hypothesis and the development of all assertions which are necessarily included in the terms of a proposition *without drawing on any other axioms or theorems for evidence.* It is concerned only with the purely logical process, by which we make two assertions put together show their joint meaning, and express what, without deduction, they only imply." †

The mathematical eye looks at the matter reasoned upon, it examines the principles employed, it enquires in what way they are related to one another and to fundamental ideas; it seeks to determine whether the course of the argument is the most direct that can be taken.

---

* Cajori, *History of Mathematics*, Second Edition, p. 331.

† De Morgan, *Connexion of Number and Magnitude*, p. 55. (To be referred to later as De M. I.)

"In addition to the purely logical process of extracting implied meanings out of the expressions of the hypothesis, it appeals to propositions which are not in the hypothesis, and which, for anything the hypotheses tell us to the contrary, may or may not be true. Of course—not logic—but—reason requires that these propositions should have been previously proved, or assumed, on their own evidence expressly." *

In De Morgan's mind there was no conflict between the two studies, for he was not only a great mathematician, but a master of logic as well.

My master, the late Professor Henrici, to whom I owe a great debt, told me that one day, when the conversation in the Common Room at University College had turned upon De Morgan, he expressed the view that De Morgan's writings impressed him as those of one whose primary interest was in logic, whilst his interest in mathematics was due to the fact that it gave him such abundant opportunities for applying and testing his logical theories. Whereupon one of those present, who had known De Morgan in the flesh (I had not that honour) said, "He told me so himself."

4. It was the logical nature of Euclid's argument that first attracted De Morgan's attention. In early manhood he wrote a little book entitled *Connexion of Number and Magnitude*, with the significant sub-title, "An Attempt to explain the Fifth Book of Euclid's Elements"—significant because it implies the difficulty of the argument, to which he returns again and again in his work, and also because he describes it as an "attempt," pointing to the fact that he was not altogether satisfied.

Five years later we find him writing a remarkable article in the *Penny Cyclopaedia*. † This is written from the mathematical standpoint, that is, he tries not merely to draw conclusions from the definitions laid down, but to explain them.

In one passage he constructs a certain object, namely, a row of equidistant columns, and a parallel row of equidistant railings, and a model thereof. From a consideration of the relation of the model to the object, he obtains, without drawing upon any definition of proportion, or anything more than the conception we have of that term prior to any definition (and with which we must show the agreement of any definition that we adopt), the conditions which Euclid lays down in the Fifth Definition, thus bringing the Definition into relation with our fundamental ideas.

A complete description of the object and model, to which I can add nothing, will be found in Heath's great edition of the *Elements of Euclid, monumentum aere perennius*,‡ therefore I will leave you to read this at your leisure.

5. Another passage in this article is even more noteworthy, but it has been apparently overlooked by mathematicians and historians of mathematics.

In it De Morgan notes that one of the three sets of conditions § laid down in the Fifth Definition can be deduced from the other two.

He gives a demonstration which did not satisfy even De Morgan himself, for he appends to it this extraordinary footnote, "We leave the reader to put this demonstration into a more exact form." ‖

The theorem was rediscovered by Stolz. ¶ His proof depends upon a limiting process.

The simplest proof, however, which I shall give a little later, is due to Professor A. C. Dixon and Mr. E. Budden. **

---

* De Morgan, I, pp. 55-56.

† *Penny Cyclopaedia*, vol. xix (1841). (To be referred to later as De M. II.)

‡ Vol. ii, pp. 121-124.

§ See Article 21.

‖ *Penny Cyclopaedia* (1841), vol. xix. p. 52, col. 2.

¶ *Vorlesungen über allgemeine Arithmetik* (1885), part i, p. 87.

** See my *Contents of the Fifth and Sixth Books of Euclid* (Second Edition), p. 29; my *Theory of Proportion*, p. 37; and the *Cambridge Philosophical Transactions*, vol. xxii, No. x. p. 187 (1917).

Logical Analysis of Euclid's Theory of Proportion.

6. Euclid's Theory of Proportion is contained in the Fifth Book, and in some of the earlier propositions, in particular the fifth and sixth propositions, of the Tenth Book.

There are 18 Definitions and 25 * Propositions in the Fifth Book.

The first remark to be made regarding the Definitions is the absence of any useful definition of Ratio, for it is impossible to make any use of Euclid's Third Definition, which reads as follows: "A *Ratio* is a sort of relation in respect of size between two magnitudes of the same kind."

With regard to the absence of a useful definition of ratio, De Morgan says: "It is implied in the Fifth Book that wherever two magnitudes (of the same kind) exist, their joint existence gives rise to a third magnitude, called their ratio, of which magnitude no conception is given except what is contained in certain directions how to apply the terms equal, greater and less, to two of the kind. On this, the natural question is, what sort of magnitude is this, and how do we know that there is *any* magnitude *whatsoever* which admits of this apparently arbitrary exposition of definitions? This question is very much to the point, and the want of an answer *at the outset* is a main cause of the *difficulty* of the Fifth Book. The answer implied in the work of Euclid is this: Let us first consider what will follow if there be such things as ratios, or magnitudes to which these definitions of equal, greater and less apply; we shall then show (in the Sixth Book) that there are different pairs of magnitudes, of which it may be said that they have ratios, *and we shall never have occasion to inquire what ratio is.*" †

Obviously this is a view that can be taken by a mature mind, interested in reasoning for reasoning's sake. To the student it will seem that he is being asked to erect a structure without a real foundation, as baseless as the fabric of a dream.

7. All but three of the definitions are formal. These three are the Fourth, Fifth and Seventh Definitions, and are the foundation stones of the structure of Euclid's argument.

(*a*) The Fourth Definition is as follows: "Magnitudes are said to *have a ratio* to one another which are capable, when multiplied, of exceeding one another."

Euclid regards two magnitudes A and B as being of the same kind, when, s and r being *any* two positive whole numbers *whatever*, it is possible to decide whether sA is greater than, or equal to, or less than rB.

He also assumes in the Sixth Proposition of the Tenth Book that if B be any magnitude, and s any positive whole number, another magnitude G, of the same kind as B, exists, such that B is equal to sG.

The so-called Axiom of Archimedes is usually stated in the following form: If A and B be magnitudes of the same kind, then a whole number n exists, such that nA is greater than B.

In this form it is quoted in the Eighth Proposition of the Fifth Book, but it is plainly implied in the Fourth Definition, and is, in fact, only the refinement of the Arabian proverb that "it is the last straw which breaks the camel's back."

Thus the attribution of the Axiom to Archimedes, who was born in 287 B.C., and was killed in the siege of Syracuse in 212 B.C., is a misnomer, as Euclid wrote in Alexandria in the time of the first Ptolemy, who reigned from B.C. 323 to B.C. 285.

8. (*b*) The Fifth Definition lays down three *sets* of conditions which must be satisfied when two ratios are the same (or equal).

---

* It will be supposed in what follows that the logical errors in Propositions 10 and 18 pointed out by Simson have been removed.

† De Morgan, I. p. 62.

Of this Definition, De Morgan says in a logical analysis; "We have nothing to do with the reasons for this definition, *or the accordance of its parts with each other*, or with any notion of ratio, more than is contained in it. We are merely now concerned to know what follows from this definition."

9. (*c*) The Seventh Definition states a condition which must be satisfied when one ratio is greater than another.

The Fifth and Seventh Definitions are subjected to an exhaustive analysis by De Morgan.

* He constructs a great scheme of 81 alternatives, wherein those of you who delight in logic may revel. If you survive the hardships of his road, you will arrive with him at his conclusion that these alternatives may be reduced to three:

(1) The cases in which one ratio is greater than another.
(2) The cases in which one ratio is the same as another.
(3) The cases in which one ratio is less than another.

10. Of the twenty-five propositions, five deal with magnitudes and their multiples (not with ratios).

11. Of the twenty remaining propositions, fourteen are concerned with properties of *Equal* ratios, and six † with properties of *Unequal* ratios.

12. Of the fourteen propositions dealing with *Equal* ratios, seven ‡ are independent of the so-called Axiom of Archimedes.

In the case of each of these seven propositions, with which we may include the Proposition marked *B* by Simson, the object is, having given certain data, to prove the equality of two ratios.

The proof of each proposition is, or may be, obtained directly from the Fifth Definition alone, and so each can be regarded as a corollary of the Fifth Definition.

The proofs of these may be regarded as the normal or standard form of the proof that two ratios are equal (see also Art. 25).

13. Of the remainnng seven propositions, the 16th, 22nd and 23rd require special notice. As in the case of the previously mentioned seven propositions, the object is to prove that certain ratios are equal, but to do this, Euclid employs, in addition to the Fifth Definition, Archimedes' Axiom, the Seventh Definition and six other propositions, involving properties of *Unequal* ratios.

It is in Euclid's treatment of these three propositions, viz.:

Prop. 16: If A, B, C, D are all magnitudes *of the same kind*,

and if $(A : B) = (C : D)$,
then $(A : C) = (B : D)$.

Prop. 22: If $(A : B) = (T : U)$,
and if $(B : C) = (U : V)$,
then $(A : C) = (T : V)$.

Prop. 23: If $(A : B) = (U : V)$,
and if $(B : C) = (T : U)$,
then $(A : C) = (T : V)$.

that the whole, or almost the whole difficulty of his argument centres. It is always very hard to know which one of the principal definitions, the Fifth or the Seventh, and which of the propositions on *Unequal* ratios is to be used at any stage of the argument, thus making it almost impossible to obtain a clear view of the reasoning as a whole.

---

* De Morgan, I. pages 25-27.

† Nos. 8 (the latter part), 10 as corrected by Simson, 13, 14, 20 and 21.

‡ Nos. 4, 7, 11, 12, 15, 17, and 18 as corrected by Simson.

14. The proof of the Ninth Proposition requires, in addition to the Fifth Definition, the Seventh Definition and one of the propositions on *Unequal* ratios, but is not difficult.

15. Having now accounted for eleven of the propositions on *Equal* ratios, there remain three others, viz. the Nineteenth, Twenty-fourth and Twenty-fifth Propositions.

These are proved by Euclid by making use of the results of some of the eleven propositions on *Equal* ratios referred to, *without reference to the Fifth and Seventh Definitions, or the propositions on Unequal ratios.*

16. It remains only to mention Propositions Five and Six of the Tenth Book.

The Fifth Proposition is as follows: "Commensurable magnitudes have to one another the ratio which a * number has to a * number."

The Sixth Proposition is the converse of the Fifth.

Euclid's proofs of these propositions depend *inter alia* on the Twenty-second Proposition of the Fifth Book. This means that, using the Fifth and Seventh Definitions of the Fifth Book as his original starting-point, he obtains, as a consequence, the fundamental fact set forth in these propositions, on which any analytical theory of ratio must be constructed.

The logician has done his work. When the corrections of the 10th and 18th propositions, due to Simson, have been made, the logical chain cannot be broken. De Morgan says † :—"This same Book, and the *Logic of Aristotle* are the two most unobjectionable and unassailable treatises that ever were written," but of De Morgan it may be said, as of Dean Middleton, that

> "This man who was so wary held and wise,
> That as 'twas said, he scarce received
> For Gospel what the Church believed,"

yet had a superstition of his own, and this was that an argument which is *logically* perfect is perfect.

A similar opinion will be found in Sir T. L. Heath's edition of *Euclid*, vol. ii, bk. v, p. 186, where he writes that Simson at the end of his notes to the Fifth Book adds the following paragraph which deserves quotation word for word: "The Fifth Book being thus corrected I most readily agree to what the learned Dr. Barrow says, 'that there is nothing in the whole body of the elements of a more subtile invention, nothing more solidly established and more accurately handled than the doctrine of proportionals.' And there is some ground to hope that geometers will think that this could not have been said with as good reason, since Theon's time till the present."

Heath adds that "Simson's claim herein will readily be admitted by all readers who are competent to form a judgment upon his criticisms and elucidations of Book Five."

And yet, if we look at the argument as a whole, whilst we admire the subtlety of the reasoning, we note with amazement that the fundamental idea, out of which the whole Theory of Ratio springs, is deduced as a consequence of a long argument, based on the very complicated Fifth and Seventh Definitions of the Book. This very evident inversion of the natural order of thought leads us to the conclusion that the last word has not been said. Following up this idea, I proceed to suggest a rearrangement of the argument from the mathematical point of view, which does not depart from Euclidean principles or rigour.

Mathematical Analysis.

17. The mathematical eye perceives six obstacles to the acquisition of Euclid's ideas, arising out of the arrangement of his argument and the form of his text.

---

* A "number" here means a "whole number."

† De Morgan, I. p. 1.

(1) At the time the Book was written, there was no suitable algebraic notation for numbers. It is even thought by some authorities that the idea of the Irrational Number had not been formed, or at any rate had not been formulated.

(2) The fundamental ideas upon which a theory of ratio can be constructed are not set out explicitly in the forefront of the argument.

(3) The absence of any definition of ratio that can be used.

(4) No explanation is given of the fundamental Fifth and Seventh Definitions.

(5) It is not explained that the three sets of conditions for the equality of two ratios, set forth in the famous Fifth Definition, can be reduced to two by the aid of the Axiom of Archimedes.

(6) The introduction of the Seventh Definition (the test for distinguishing the greater of two unequal ratios from the smaller) into the argument is unnecessary for the proof of properties of equal ratios, which is the real object of the Book.

Here let me recount a personal incident. About thirty years ago, after having taught, or perhaps it would be more accurate to say, having attempted to teach the substance of Euclid's argument to Intermediate Students, I sat down to write an account of the views I had then formed. While so engaged, I was called away one day on business. On my way home I was waiting for a train on a suburban railway platform. The night was pitch dark. Suddenly there passed through my brain, just as if a lightning flash had illuminated the physical darkness around me, the question "*Why does Euclid use properties of Unequal ratios to prove properties of Equal ratios?*"

"Surely if his test for the equality of ratios is sound and complete, it should be sufficient to prove all properties of *Equal* ratios by itself, without leaning for support on the test for distinguishing between *Unequal* ratios?"

In great excitement I hurried to a waiting-room, where a feeble gas-jet was burning, and wrote down the demonstration of the Sixteenth Proposition which I shall give you presently, which showed me that I had found a key to Euclid's treasure-house.

18. *Obstacle* (1). The ancients had no general notation for the positive whole number, or for the positive rational fraction which we denote by the quotient of one positive integer by another. Such numbers are called positive rational numbers, but besides these there is another class of positive numbers which had, perhaps, not been clearly recognised by the ancients. These are magnitudes *of the same kind* as the positive rational numbers, *i.e.* it is imagined that it is possible to decide whether any such number is greater or is less than *any* rational number *whatever*. Dedekind put the matter thus: "An irrational number is defined by the specification of all rational numbers that are less, and all those that are greater than the number to be defined." *

Thus the rule for defining the square root of 2 is as follows:

If r and s are two positive integers, such that $2s^2$ is greater than r^2, then the square root of 2 is greater than $\frac{r}{s}$; but if $2s^2$ is less than r^2, then the square root of 2 is less than $\frac{r}{s}$. And since $2s^2$ can never be equal to r^2, the above rule makes it possible to distinguish all those rational numbers which are less than the square root of 2 from those which are greater than it.

19. *Obstacle* (2). The fundamental ideas on which any analytical theory of ratio must be based are not stated at the beginning of the Book.

These fundamental ideas can be obtained thus: Suppose that we are considering two magnitudes of which we can, or think we can, form a judgment that they are equal or unequal, and in the latter case, which is the greater; such as two lengths or two areas or two weights.

* Dedekind, *The Nature and Meaning of Numbers*, p. 39 of the English Translation.

For example, suppose that the magnitudes are

two inches and three inches,
or two acres and three acres,
or two pounds and three pounds.

There is something which is common to all these three different cases. In each case, when we think of the relation of one of these magnitudes to the other, there arises in our minds the thought of the relation of the number 2 to the number 3.

We define this relation to be the rational fraction or number $\frac{2}{3}$, which we call the ratio of the first magnitude to the second.

In general, suppose that G is any magnitude, and a, b, are any two whole numbers; and let $A=aG$, $B=bG$.

Then, considering the relation of A to B, this is the same as the relation of aG to bG.

This gives rise in our minds to a third magnitude, viz. the rational number $\frac{a}{b}$ (which is independent of the nature of G).

We express this with the usual notation for ratio, thus:

$$(A : B)=(aG : bG)=\frac{a}{b}. \quad \text{.....(I)}$$

It should be noted for use later that, in this case,

$$bA=b(aG)=a(bG)=aB;$$

i.e. there exists a multiple of A which is also a multiple of B.

Let us suppose that c is another whole number, and that there is a magnitude $C=cG$.

Then
$$(B : C)=(bG : cG)=\frac{b}{c}.$$

Working with such magnitudes as A, B, C, all of which are multiples of the same magnitude, which we have called G, we soon arrive at the conclusions that

If $A=B$, then $(A : C)=(B : C)$, and $(C : A)=(C : B)$. .....(II)

If $A>B$, then $(A : C)>(B : C)$,
If $A<B$, then $(A : C)<(B : C)$. } .....(III)

It is to be noted that the two inequalities in (III) do not express facts really distinct from one another. They have therefore been placed together.

These conclusions have been reached on the hypothesis that A, B, C are all multiples of the same magnitude G.

The next step is to *generalise* the results (II) and (III), and to *assume* that whether A, B, C are all multiples of the same magnitude or not, the term "ratio" is to be so defined that the equations (I) and (II) and the inequalities (III) are always satisfied. *These are the real foundations on which any analytical theory of ratio must rest.*

They appear under a very different form in Euclid's work, viz. as propositions deduced as consequences of his Fifth and Seventh Definitions, on which his work is based.

The idea marked (I) will be found in the fifth proposition of the Tenth Book, where it is stated thus:

"Commensurable magnitudes have to one another the ratio which a number* has to a number."

Euclid's proof depends on the Twentieth Definition of the Seventh Book and the Twenty-second Proposition of the Fifth Book.

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* A "number" here means a "whole number."

The equalities marked (II) are proved in Euclid V. 7.

The inequalities marked (III) are proved in Euclid V. 8.

This is a complete inversion of the natural order of thought, for the ideas (I), (II), (III) are fundamental, and cannot be proved in any proper sense of the term.

It will be shown presently how to reverse the process, and to deduce the Definitions, on which Euclid's work is based, from these three fundamental ideas.

20. *Obstacle* (3). I must first show how to define as a number the ratio of any two magnitudes *of the same kind*, when they have no common measure.

It was remarked in the last article that when two magnitudes of the same kind, A, B, had a common measure, then a certain multiple of A happened to be equal to a multiple of B.

Let us then take any two magnitudes A, B, and suppose it is unknown whether they have or have not a common measure.

Take any multiples of A and B, say sA and rB. Then one of the following three things must happen:

(i) $sA > rB$, or (ii) $sA = rB$, or (iii) $sA < rB$.

It is assumed as an axiom (see *Euclid*, x, 6), that a magnitude G exists, of the same kind as B, such that $B = sG$.

Then, (i) if $sA > rB$, $sA > r(sG)$;

$\therefore\ sA > s(rG)$;

$\therefore\ A > rG$;

$\therefore\ (A : B) > (rG : B)$; .....by (III)

$\therefore\ (A : B) > (rG : sG)$; ..... by (II)

$\therefore\ (A : B) > \frac{r}{s}$. .....by (I)

(ii) If $sA = rB$, $sA = r(sG)$, $sA = s(rG)$;

$\therefore\ A = rG$;

$\therefore\ (A : B) = (rG : sG)$; .....by (II)

$\therefore\ (A : B) = \frac{r}{s}$. .....by (I)

(iii) If $sA < rB$, $sA < r(sG)$, $sA < s(rG)$, $A < rG$;

$\therefore\ (A : B) < (rG : sG)$; .....by (III)

$\therefore\ (A : B) < \frac{r}{s}$. .....by (I)

Hence, if (i) $sA > rB$, then $(A : B) > \frac{r}{s}$,

if (ii) $sA = rB$, then $(A : B) = \frac{r}{s}$,

if (iii) $sA < rB$, then $(A : B) < \frac{r}{s}$.

From these it follows conversely that

if (iv) $(A : B) > \frac{r}{s}$, then $sA > rB$,

if (v) $(A : B) = \frac{r}{s}$, then $sA = rB$,

if (vi) $(A : B) < \frac{r}{s}$, then $sA < rB$.

These six results furnish a rule for determining whether the ratio $(A : B)$ is greater than, or equal to, or less than *any* rational number *whatever*; consequently, in accordance with Dedekind's definition, the ratio $(A : B)$ is to be regarded as a number.

21. *Obstacle* (4). The Definitions on which Euclid's Work is based, viz. the Fifth and the Seventh, are not explained in such a manner as to bring them into connection with fundamental ideas.

The Fifth Definition is the test for determining when two ratios are equal.

Let the ratios be $(A : B)$ and $(C : D)$.

Then as they are equal, they must be either both equal to the same rational number, or else no rational number can fall between them.

Case I. Suppose that $(A : B) > \frac{r}{s}$,

then must also $(C : D) > \frac{r}{s}$,

i.e. by the preceding article,

if $sA > rB$, then also $sC > rD$. .....(I)

Case II. If, however, $(A : B) = \frac{r}{s}$,

then must $(C : D) = \frac{r}{s}$,

i.e. by the preceding article,

if $sA = rB$, then must $sC = rD$. .....(II)

Case III. If $(A : B) < \frac{r}{s}$,

then must $(C : D) < \frac{r}{s}$,

i.e. by the preceding article,

if $sA < rB$, then must $sC < rD$. .....(III)

The conditions (I), (II) and (III) are the three *sets* of conditions of Euclid's famous Fifth Definition.

I do not stop to explain the Seventh Definition, as it will not be required in the argument when rearranged as here suggested.*

22. *Obstacle* (5). In the case of those propositions whose proofs involve the Axiom of Archimedes. there is a special difficulty in proving that the second set of conditions in the Fifth Definition is satisfied. It is here that there appears the capital importance of De Morgan's remark that if the first and third sets of conditions are satisfied in all cases, then the second set of conditions is also satisfied.

A proof is given below.

Referring to the three sets of conditions in the Fifth Definition, given in Article 21, let us suppose that the first and third sets are satisfied for all integral values of r and s.

Suppose further that integers t and u exist, such that $tA = uB$.

It is required to prove that $tC = uD$.

For if $tC \neq uD$, then either $tC > uD$, or $tC < uD$.

Suppose $tC > uD$, then by Archimedes' Axiom, a whole number n exists, such that

$$n(tC - uD) > D;$$

$$\therefore\ ntC > (nu + 1)D.$$

* It can be deduced from the consideration that, if two ratios are unequal, then some rational fraction falls between them, or is equal to one of them but not equal to the other.

But $$ntA = nuB;$$

$$\therefore\ ntA < (nu+1)B,$$

whilst $$ntC > (nu+1)D.$$

These two inequalities do not satisfy Condition (III) of Article 21,

when $$s = nt \quad \text{and} \quad r = (nu+1).$$

Hence $$tC \not> uD.$$

If, however, $$tC < uD,$$

then an integer n exists, such that

$$n(uD - tC) > C;$$

$$\therefore\ (nt+1)C < nuD,$$

but $$ntA = nuB;$$

$$\therefore\ (nt+1)A > nuB,$$

whilst $$(nt+1)C < nuD.$$

These two inequalities do not satisfy Condition (I) of Article 21,

when $$s = (nt+1) \quad \text{and} \quad r = nu.$$

Hence $$tC \not< uD.$$

And it was shown that $$tC \not> uD;$$

$$\therefore\ tC = uD.$$

Consequently, when using the Fifth Definition, it is sufficient to show that the sets of conditions (I) and (III) are satisfied in all cases.

In what follows this will be referred to as the *simplified* form of the Fifth Definition.

It is of great importance in simplifying the proofs of Propositions 16, 19, 22, 23, and 24, because the proof of these propositions is peculiarly difficult, when the second set of conditions in the Fifth Definition has to be shown to be satisfied.

Neither this proof nor Stolz's can be regarded as obtainable from the original definition by logical reasoning only, inasmuch as Archimedes' Axiom has been employed.

23. *Obstacle* (6). The Seventh Definition, the test for distinguishing the greater from the smaller of two unequal ratios, together with six Propositions (Nos. 8,* 10, 13, 14, 20 and 21) on *Unequal* ratios, are not necessary for the proof of all the properties of *Equal* ratios which Euclid gives.

The whole of the information given by them can be deduced from the simplified form of the Fifth Definition, and the following theorem, which forms

The Earlier Part of Euclid V. 8.

24. This proposition is as follows:

Let X, Y, Z be magnitudes of the same kind.

Let $$X - Y > Z.$$

To prove that a whole number, q exists, such that

$$X > qZ > Y.$$

Let qZ be the *smallest* multiple of Z which exceeds Y.

Then either $qZ - Y = Z$, or $qZ - Y < Z$.

$$\therefore \text{ in either case, } X - Y > qZ - Y;$$

$$\therefore\ X > qZ \quad \text{and} \quad qZ > Y.$$

* It will be seen that a portion of the earlier part of this proposition, which part, however, does not contain any reference to ratio, will be required.

I will put this proposition in a homely form. Let us suppose that the distance from the College door to the nearer bank of the Thames is represented by Y.

Let the distance from the College door to the farther bank of the Thames be represented by X.

Let Z represent the length of your stride, then $X - Y$, which represents the breadth of the river, is greater than Z, the length of your stride.

Then the proposition expresses nothing more than this, that at the qth stride after leaving the College door, you will fall into the river.

The case in which $qZ - Y = Z$ and therefore $(q-1)Z = Y$, is the case in which the $(q-1)$th stride brings you *exactly* up to the nearer bank of the river.

The case in which $qZ - Y < Z$, and therefore $(q-1)Z < Y$, whilst $qZ > Y$, is the case in which the $(q-1)$th stride *does not bring you exactly up to the nearer bank of the river, but does bring you within a distance from it less than the length of your stride.*

Euclid draws the following consequence :

If A, B, C, be magnitudes of the same kind, and if $A > B$, then, by Archimedes's Axiom, an integer n exists, such that $n(A-B) > C$;

$$\therefore\ nA - nB > C.$$

Hence, using the proposition just proved, and putting therein,

$$X = nA, \quad Y = nB, \quad Z = C,$$

it follows that an integer q exists, such that

$$nA > qC > nB.$$

The proof of these two results occupies 48 lines of Heath's translation of Euclid's Text. It shows us how great an obstacle the want of a suitable notation was to Euclid.

25. Having noted these obstacles, I proceed to set forth a rearrangement of the argument, which may be regarded at the same time as a mathematical analysis, and a commentary on Euclid's Theory.

The Theory, as set forth here, depends on the Axiom of Archimedes and Euclid's Fifth Definition.

[The Seventh Definition is not required.

Unequal ratios are involved in the third of the fundamental ideas on which the theory of ratio is constructed. (Article 19.)

They are also involved in the formulation of the rule for defining ratio as a number. (Article 20.)

Consequently their use is implied whenever this rule is used in the proofs of the propositions.

This rule is however used always in the same way and presents no difficulty comparable with that involved in Euclid's use of the Seventh definition.]

In the first place there are five propositions on magnitudes and their multiples, not ratios. (See Art. 10 and Appendix I.)

Then there are seven Propositions (Nos. 4, 7, 12, 15, 17, 18 and the proposition marked B by Simson *), the character of which has been set forth fully in Article 12. (See also Appendix I.)

These are proved as follows.

Having given certain data, suppose it is required to prove that

$$(A : B) = (C : D).$$

Then we compare $(A : B)$ with *any* rational fraction *whatever*, $\frac{r}{s}$.

* It is not necessary to include Proposition 11 in this list, since ratio has been defined as a number, and this proposition expresses nothing more than that numbers which are equal to the same number are equal to one another.

There are three alternatives:

$$\text{(i)}\ (A:B)>\frac{r}{s},$$

$$\text{(ii)}\ (A:B)=\frac{r}{s},$$

$$\text{(iii)}\ (A:B)<\frac{r}{s}.$$

Consider (i), $(A:B)>\frac{r}{s}$;

$$\therefore\ sA>rB.$$

Then it is proved from the data of the proposition that

$$sC>rD;$$

$$\therefore\ (C:D)>\frac{r}{s}.$$

(ii) In the second case in like manner it is proved that

when $(A:B)=\frac{r}{s}$,

then $(C:D)=\frac{r}{s}$.

(iii) When $(A:B)<\frac{r}{s}$,

it is proved that $(C:D)<\frac{r}{s}$.

Hence $\frac{r}{s}$, which represents *any* rational number *whatever*, cannot lie between $(A:B)$ and $(C:D)$;

$$\therefore\ (A:B)=(C:D).$$

26. Euclid gives in the Sixth Book two examples of this method of proof.

In the first proposition he supposes that A and B represent the areas of two parallelograms of the same altitude, and that C and D are the lengths of their respective bases.

Then he shows that $(A:B)=(C:D)$.

A like proof holds for triangles of the same altitude.

In the thirty-third proposition he supposes that A and B are angles at the centres of two equal circles, and that C and D are the lengths of the arcs subtended by these angles (or the areas of the sectors of these circles, whose angles are A and B).

Then he shows that $(A:B)=(C:D)$.

He might also have substituted for his second proposition, which he deduces from the first, the following:

Suppose that two straight lines in a plane are crossed by any number of parallel lines, and let A and C be the segments of the two lines made by some pair of parallel lines, and let B and D be in like manner the segments of the two lines (taken in the same order) between some other pair of the parallel lines. Then he could have proved in the same way that

$$(A:B)=(C:D).$$

27. We come now to three important propositions, viz.:

Proposition 16. If $(A:B)=(C:D)$, and if all the magnitudes are of the same kind, then

$$(A:C)=(B:D).$$

Proposition 22. If $(A : B) = (T : U)$,
and if $(B : C) = (U : V)$,
then $(A : C) = (T : V)$.

Proposition 23. If $(A : B) = (U : V)$,
and if $(B : C) = (T : U)$,
then $(A : C) = (T : V)$.

We shall now show that these three propositions can be proved by the aid of De Morgan's simplification of the Fifth Definition (Article 22), and by the earlier part of the Eighth Proposition (Article 24), without reference to the Seventh Definition, or the six propositions depending on it.

Except for the single step in which the earlier part of Proposition Eight is used, these proofs follow the standard form of the seven propositions already referred to in Article 25.

Euclid V. 16.

28. If A, B, C, D be magnitudes of the same kind, and if

$$(A : B) = (C : D),$$

to prove that $(A : C) = (B : D)$.

Compare $(A : C)$ with *any* rational fraction *whatever* r/s.
There are three possibilities

$$(A : C) > r/s, \quad (A : C) = r/s, \quad (A : C) < r/s.$$

By de Morgan's Theorem we know that we need only consider the first and last.

(i) Let $(A : C) > r/s$,
then $sA > rC$.

Then an integer n exists such that $n(sA - rC) > B$;

$\therefore\ nsA - nrC > B$;

$\therefore$ an integer q exists such that $nsA > qB > nrC$; *

Since $nsA > qB$,

$(A : B) > q/ns$.

But $(A : B) = (C : D)$;

$\therefore\ (C : D) > q/ns$;

$\therefore\ nsC > qD$, $\quad qB > nrC$

$rnsC > rqD$; $\quad sqB > snrC$.

$\therefore\ sqB > rqD$,

$sB > rD$,

$(B : D) > r/s$.

(ii) Let $(A : C) < r/s$,

$sA < rC$;

$\therefore$ an integer n exists such that $n(rC - sA) > B$;

$\therefore\ nrC - nsA > B$;

$\therefore$ an integer q such that $nrC > qB > nsA$;

$\therefore\ nsA < qB < nrC$; †

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* The argument up to this point is the same as that required for the proofs of Propositions 22, 23. It will be noted that it is at this stage that the earlier part of Euc. V. 8 has been used.

† It will be seen that the rest of the argument in this case is obtainable from the corresponding portion of the argument in the previous case by reversing the sign of inequality.

Since $nsA < qB$,

$(A : B) < q/ns$.

But $(A : B) = (C : D)$;

$\therefore (C : D) < q/ns$;

$\therefore nsC < qD$, $\quad qB < nrC$,

$rnsC < rqD$, $\quad sqB < snrC$.

$\therefore sqB < rqD$,

$sB < rD$,

$(B : D) < r/s$.

Hence r/s, which represents *any* rational number *whatever*, cannot lie between

$(A : C)$ and $(B : D)$;

$\therefore (A : C) = (B : D)$.

EUCLID V. 22.

29. If $(A : B) = (T : U)$,

and if $(B : C) = (U : V)$,

to show that $(A : C) = (T : V)$.

The proof of this proposition is the same as that of the sixteenth proposition up to the point indicated by the *.

Resuming at this point, it has been shown that if

$(A : C) > r/s$

then integers n and q exist such that

$nsA > qB$;

$\therefore (A : B) > q/ns$,

but $(A : B) = (T : U)$;

$\therefore (T : U) > q/ns$;

$\therefore nsT > qU$.

$qB > nrC$;

$\therefore (B : C) > nr/q$,

but $(B : C) = (U : V)$,

$\therefore (U : V) > nr/q$,

$\therefore qU > nrV$.

$\therefore nsT > qU > nrV$;

$\therefore nsT > nrV$;

$\therefore sT > rV$;

$\therefore (T : V) > r/s$.

In like manner, if $(A : C) < r/s$, it can be shown that

$(T : V) < r/s$.

$\therefore r/s$, which represents *any* rational fraction *whatever*, cannot lie between

$(A : C)$ and $(T : V)$;

$\therefore (A : C) = (T : V)$.

30. EUCLID V. 23.

If $(A : B) = (U : V)$,

and if $(B : C) = (T : U)$,

to prove that $(A : C) = (T : V)$.

The proof of this proposition is the same as that of the sixteenth up to the point indicated by the *.

Resuming at this point, it has been shown that if $(A : C) > r/s$, integers n and q exist such that

$$\begin{aligned} nsA &> qB; \\ \therefore\ (A : B) &> q/ns, \\ \text{but}\quad (A : B) &= (U : V); \\ \therefore\ (U : V) &> q/ns; \\ \therefore\ nsU &> qV; \\ \therefore\ rnsU &> rqV. \end{aligned}$$

$$\begin{aligned} qB &> nrC; \\ \therefore\ (B : C) &> nr/q, \\ \text{but}\quad (B : C) &= (T : U); \\ \therefore\ (T : U) &> nr/q; \\ \therefore\ qT &> nrU; \\ \therefore\ sqT &> snrU; \end{aligned}$$

$$\begin{aligned} \therefore\ sqT &> rqV; \\ \therefore\ sT &> rV; \\ \therefore\ (T : V) &> r/s. \end{aligned}$$

In like manner it can be shown that if

$$(A : C) < r/s,$$

then

$$(T : V) < r/s.$$

$\therefore$ r/s, which represents *any* rational number *whatever*, cannot lie between

$$(A : C) \quad \text{and} \quad (T : V);$$

$$\therefore\ (A : C) = (T : V).$$

31. Proofs of these three propositions in a slightly different form were published in the *Transactions of the Cambridge Philosophical Society* in 1897.

The proofs in the form here given were published in the *second* edition of my *Contents of Euclid Books V. and VI* (Cambridge University Press), 1908, and in my *Theory of Proportion* (Constable & Co.), 1914.

So far as I am aware, these proofs have escaped the notice of mathematicians and historians of mathematics.

The substitution of these three proofs for those given by Euclid makes it unnecessary to introduce into the argument the Seventh Definition and the six Propositions dependent on it, and this substitution is the most important constructive criticism of the Euclidean argument in this Address.

32. We have now accounted for ten propositions, giving properties of *Equal* ratios. There remain four properties of *Equal* ratios, given in Propositions 9, 19, 24 and 25.

All these may be deduced (*without reference to the fundamental Definitions*) from the ten propositions, which, as here arranged, follow the normal form of proof.

The best proofs of the 19th, 24th, and 25th propositions are those given by Euclid. I have given elsewhere * the proofs of the 19th and 24th in the normal form, but these are difficult.

The ninth and the twenty-fifth propositions are not of the same type as the twelve propositions already referred to, for in each of these it is given that two ratios are equal, and it has to be proved that certain relations between the magnitudes involved exist.

It cannot, therefore, be expected that the proofs should conform to the normal type of the ten propositions.

33. The proof of the ninth proposition is as follows:

If $(A : C) = (B : C)$ or if $(C : A) = (C : B)$, then $A = B$.

Making use of Euc. V. 16, which has been proved in Art. 28, then the two proportions supposed to hold good are equivalent to $(A : B) = (C : C)$ and $(C : C) = (A : B)$ respectively.

But

$$(C : C) = 1;$$

$$\therefore\ (A : B) = 1;$$

$$\therefore\ A = B.$$

* *Camb. Phil. Trans.*, vol. xxii. No. xxiv, p. 460; *Camb. Phil. Soc. Proc.*, vol. xxi, pt. v, p. 474; *Camb. Phil. Soc. Proc.*, vol. xxiii, pt. vii, p. 779.

SUMMARY OF THE ARGUMENT.

34. Irrational Numbers having been defined, and it having been explained what is meant by magnitudes *of the same kind*, the fundamental ideas on which any Algebraic Theory of Ratio must be based are set forth.

The rule for defining the ratio of any two magnitudes *of the same kind* as a number (Rational or Irrational) is then given.

From this, Euclid's Definition of Proportion is deduced.

From this Definition seven Propositions concerning Equal Ratios are deducible. The proof of each may be arranged independently of all the others, and they all follow the same lines.

Next, De Morgan's Simplification of the Definition of Proportion by the aid of the Axiom of Archimedes is demonstrated, and the following Proposition, contained in the earlier part of Euclid V. 8, viz. "If X, Y, Z are magnitudes of the same kind, and if $X - Y > Z$, then a multiple of Z, say qZ, exists, such that $X > qZ > Y$" is proved.

From this Proposition, together with the simplified form of the Definition of Proportion, and Archimedes' Axiom, it is shown that the proofs of the 16th, 22nd and 23rd Propositions of the Fifth Book can be thrown into the same form as the seven Propositions already referred to, thus making a total of ten Propositions proved on the same lines.

From the results of these ten Propositions * the proofs of the remaining four properties of Equal Ratios proved by Euclid may be deduced, *without reference to the original definitions.*

Of the four Propositions last mentioned, two are of the same type as the preceding ten, and may be proved, though with difficulty, on the same lines. The remaining two Propositions † are, however, of a different type, and so it cannot be expected that their proofs should follow the same lines as the preceding twelve.

No use is made of Euclid's Seventh Definition, which shows how to distinguish the greater of two unequal ratios from the smaller, nor of the six Propositions intimately connected with this Definition, which are *subsidiary* to the main line of Euclid's argument.

In this way, it becomes possible to obtain a view of the subject as a whole.

When so presented it is clear that the real object of the argument is to discuss the Theory of *Equal* Ratios.

35. To-day I have reached the end of my long struggle to understand the structure of Euclid's great argument and to explain it in a manner which can be understood and grasped as a whole by students of average mathematical ability.

Euclid's Text was admitted by De Morgan to be difficult, and it is useless to place it before students. It has passed out of the course of study in the Universities, and the Theory stands on a lonely pedestal, magnificent but almost unknown in the schools, unapproached and practically unapproachable.

It was from this Theory that Dedekind received the inspiration which led to his definition of Irrational Numbers, which occupies, and probably will always occupy, a place in the forefront of mathematical analysis.

The properties of Equal Ratios in the Fifth Book furnish simple applications of Dedekind's definition, and should, I believe, be included in the course of study of every mathematician.

For these reasons, I have brought the matter before you, and have ventured upon a criticism of Euclid's line of argument, my object being to make Euclid's

* In each of these Propositions, having given certain data, the object is to prove the equality of two ratios.

† In these two Propositions, it is given that two ratios are equal, and certain properties of the magnitudes appearing therein have to be proved.

ideas accessible to all students of average mathematical ability, without departing in any way from Euclidean principles or rigour.

I found that my students were able to understand and reproduce the argument in the form here set forth. The work used to occupy about ten lectures, and the unified form which was given to ten of the propositions made it possible to grasp the subject as a whole.

APPENDIX I.

Rearrangement of the Argument.

Ratio having been defined, and the conditions for the equality of two ratios having been obtained, the following is the order of the propositions : *

1. $n(A+B+C+\dots)=nA+nB+nC+\dots$. (Euc. V. 1.)
2. $(a+b+c+\dots)N=aN+bN+cN+\dots$. (Euc. V. 2.)
3. $(r(s))A=r(sA)=s(rA)=(s(r))A$.
4. If $A>B$, then $r(A-B)=rA-rB$. (Euc. V. 5.)
5. If $a>b$, then $(a-b)R=aR-bR$. (Euc. V. 6.)

Seven † Propositions Deduced from the Fifth Definition, without the Aid of Archimedes' Axiom.

6. If $(A:B)=(C:D)$,
then $(rA:sB)=(rC:sD)$. (Euc. V. 4.)

7. If $(A:B)=(C:D)$,
then $(B:A)=(D:C)$. (Euc. V. B.)

‡8. If $A=B$,
then $(A:C)=(B:C)$, and $(C:A)=(C:B)$. (Euc. V. 7.)

9. If $(A:B)=(C:D)=(E:F)$, and if all the magnitudes are *of the same kind*,
then $(A:B)=(A+C+E):(B+D+F)$. (Euc. V. 12.)

10. $(A:B)=(nA:nB)$. (Euc. V. 15.)

11. If $(A:B)=(X:Y)$,
then $(A+B):B=(X+Y):Y$. (Euc. V. 18.)

12. If $(A+B):B=(X+Y):Y$,
then $(A:B)=(X:Y)$. (Euc. V. 17.)

De Morgan's Simplification of Euclid's Fifth Definition.

13. Proof that the three sets of conditions in the Fifth Definition are reducible to two by the aid of Archimedes' Axiom.

14. (i) If X, Y, Z are magnitudes *of the same kind*, and if

$$X-Y>Z,$$

then a whole number q exists, such that

$$X>qZ>Y.$$

(ii) If A, B, C are magnitudes *of the same kind*, and if

$$A>B,$$

then integers n and q exist such that

$$nA>qC>nB.$$

} The earlier part of Euc. V. 8.

* For proofs, see my *Contents of Euclid*, Books V. and VI, and my *Theory of Proportion*.

† Euclid V. 11 is not required when ratio has been defined as a number.

‡ We have assumed this as a fundamental idea of ratio. We may also deduce it from the test for equal ratios.

THREE PROPOSITIONS WHOSE PROOFS DEPEND ON ARCHIMEDES' AXIOM, DE MORGAN'S SIMPLIFICATION OF THE FIFTH DEFINITION, AND PROPOSITION 14 ABOVE.

15. If $(A : B) = (C : D)$,

and if all the magnitudes be *of the same kind*,

then $(A : C) = (B : D)$. (Euc. V. 16.)

16. If $(A : B) = (T : U)$,

and if $(B : C) = (U : V)$,

then $(A : C) = (T : V)$. (Euc. V. 22.)

17. If $(A : B) = (U : V)$,

and if $(B : C) = (T : U)$,

then $(A : C) = (T : V)$. (Euc. V. 23.)

FOUR PROPOSITIONS DEDUCIBLE FROM THE PRECEDING.

18. If $(A + C : B + D) = (C : D)$,

then $(A : B) = (C : D)$. (Euc. V. 19.)

19. If $(A : C) = (X : Z)$,

and if $(B : C) = (Y : Z)$,

then $(A + B : C) = (X + Y : Z)$. (Euc. V. 24.)

20. If $(A : C) = (B : C)$,

or if $(C : A) = (C : B)$,

then $A = B$. (Euc. V. 9.)

21. If four magnitudes *of the same kind* are in proportion, then the sum of the greatest and least is greater than the sum of the remaining two. (Euc. V. 25.)

APPENDIX II.

GEOMETRICAL THEORIES OF PROPORTION.

It would not be right for me to pass over without reference the Geometrical Theories of Proportion due to Hilbert and Forder, which are entirely distinct from Euclid's Algebraic Theory. It would be out of place to examine them here in detail, but it is interesting to note that in these Theories, the proofs given of the propositions formally corresponding to Euc. V. 16, 22 and 23 (all the magnitudes, however, being supposed to be of the same kind), require, as in Euclid's argument, a special form of treatment, which is provided in this case by Pappus's Theorem, a particular case of Pascal's Theorem. For proofs, see *Hilbert's *Foundations of Geometry* and †Forder's *Foundations of Euclidean Geometry*.

The proof in Hilbert's book depends on the property of the orthocentre of a triangle, and consequently involves ‡ the postulate that all right angles are equal.

The proof in Forder's book depends on the properties of the cyclic quadrilateral; thus it will be found that properties of the circle, the Postulate of Parallels, axioms of incidence, axioms of order, axioms of congruence, and the postulate that all right angles are equal, are involved in the exposition of these Theories.

In the argument given in this Address, the only assumptions, besides the Axiom of Archimedes, are axioms of magnitude.

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* Hilbert's *Foundations of Geometry*, pp. 41-45.

† Forder's *Foundations of Euclidean Geometry*, p. 153.

‡ Hilbert's *Foundations of Geometry*, p. 20.

It would appear that where the Axiom of Archimedes is employed in this Theory and in Euclid's Theory, the Geometrical Theories make use of Pappus's Theorem, but it is an entirely separate question whether the results of the Geometrical Theories of *Proportion* give all the results of the Analytical Theory of *Ratio* obtainable from the Euclidean argument; *e.g.* In what way do the Geometrical Theories furnish the proofs of the following propositions:

$$(A : C)+(B : C)=[(A+B) : C]$$

and

$$(A : B)\times(B : C)=(A : C),$$

both of which require the use of Archimedes' Axiom in the Analytical Theory ?* This question I must leave to future investigators.

APPENDIX III.

Addition and Multiplication of Ratios. Addition of Ratios. Euclid's Twenty-second Datum.

To prove that $(A : C)+(B : C)=[(A+B) : C]$.

It will be supposed that all the ratios are incommensurable.

Suppose $[(A+B) : C]>\frac{r}{s}$,

then $s(A+B)>rC$.

By Archimedes' Axiom, an integer n exists, such that

$$n[s(A+B)-rC]>C;$$

$$\therefore\ ns(A+B)>(nr+1)C. \quad \text{.....(I)}$$

Suppose that $nsA>kC$, ...(II)

whilst $nsA<(k+1)C$. .....(III)

From (I) and (III) we get, by subtraction,

$$nsB>(nr-k)C. \quad \text{.....(IV)}$$

From (II), $(A : C)>\frac{k}{ns}$, ...(V)

and from (IV), $(B : C)>\frac{nr-k}{ns}$; .....(VI)

$$\therefore \text{ from (V) and (VI), } (A : C)+(B : C)>\frac{k}{ns}+\frac{nr-k}{ns};$$

$$\therefore\ (A : C)+(B : C)>\frac{r}{s} \text{ when } [(A+B) : C]>\frac{r}{s}.$$

Consider the case $[(A+B) : C]<\frac{r}{s}$,

then $s(A+B)<rC$.

By Archimedes' Axiom, an integer n exists, such that

$$n[rC-s(A+B)]>C;$$

$$\therefore\ ns(A+B)<(nr-1)C. \quad \text{.....(I')}$$

Suppose that $nsA<kC$, ...(II')

whilst $nsA>(k-1)C$. .....(III')

From (I') and (III') we get, by subtraction,

$$nsB<(nr-k)C. \quad \text{.....(IV')}$$

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* See Appendix III.

From (II′), $$(A : C) < \frac{k}{ns}, \quad \text{.....(V′)}$$

and from (IV′), $$(B : C) < \frac{nr - k}{ns}; \quad \text{.....(VI′)}$$

$\therefore$ from (V′) and (VI′), $(A : C) + (B : C) < \frac{k}{ns} + \frac{nr - k}{ns}$;

$$\therefore (A : C) + (B : C) < \frac{r}{s} \text{ when } [(A + B) : C] < \frac{r}{s}.$$

Hence $\frac{r}{s}$, which represents *any* rational fraction *whatever*, cannot lie between

$$[(A + B) : C] \text{ and } (A : C) + (B : C);$$
$$\therefore (A : C) + (B : C) = [(A + B) : C].$$

Euclid did not prove this result, but the 24th Proposition of the Fifth Book, which is as follows:

If $$(A : C) = (A' : C'),$$
and if $$(B : C) = (B' : C'),$$
then $$[(A + B) : C] = [(A' + B') : C'],$$
can be deduced from it, but the process cannot be reversed.

Multiplication of Ratios. Euclid's Formula for Compounding Ratios.

To prove that $$(A : B) \times (B : C) = (A : C).$$

Suppose that $$(A : C) > \frac{r}{s};$$
$$\therefore sA > rC.$$

By Archimedes' Axiom, an integer n exists, such that
$$n(sA - rC) > B,$$
$$nsA - nrC > B;$$

$\therefore$ an integer q exists, such that
$$nsA > qB > nrC;$$
$$\therefore (A : B) > \frac{q}{ns},$$

and $$(B : C) > \frac{nr}{q};$$

$$\therefore (A : B) \times (B : C) > \frac{q}{ns} \times \frac{nr}{q};$$

$$\therefore (A : B) \times (B : C) > \frac{r}{s}, \text{ when } (A : C) > \frac{r}{s}.$$

Suppose that $$(A : C) < \frac{r}{s};$$
$$\therefore sA < rC.$$

By Archimedes' Axiom, an integer n exists, such that
$$n(rC - sA) > B,$$
$$nrC - nsA > B;$$

$\therefore$ an integer q exists, such that

$$nsA < qB < nrC;$$

$$\therefore\ (A:B) < \frac{q}{ns},$$

and

$$(B:C) < \frac{nr}{q};$$

$$\therefore\ (A:B)\times(B:C) < \frac{q}{ns}\times\frac{nr}{q};$$

$$\therefore\ (A:B)\times(B:C) < \frac{r}{s},\ \text{when}\ (A:C) < \frac{r}{s}.$$

Hence $\frac{r}{s}$, which represents *any* rational fraction *whatever*, cannot lie between

$$(A:C)\quad\text{and}\quad(A:B)\times(B:C);$$

$$\therefore\ (A:B)\times(B:C)=(A:C).$$

Euclid did not prove this result, but the 22nd Proposition of the Fifth Book, which is as follows:

If $(A:B)=(A':B')$,

and if $(B:C)=(B':C')$,

then $(A:C)=(A':C')$

can be deduced from it, but the process cannot be reversed.

All other cases of Addition and Multiplication of ratios can be reduced to the preceding cases by means of the following Proposition:

If A and B be two magnitudes of the same kind, and if B' be any other magnitude, then a magnitude A', of the same kind as B', exists, such that

$$(A':B')=(A:B).$$

The proof of this Proposition depends on the idea of Continuity.

For proofs, see *Camb. Phil. Trans.*, vol. xvi, pt. iv. p. 244, and "Theory of Proportion," section 70, p. 76.

Conclusion.

It remains only for me to thank you for the honour which you did me two years ago in electing me to the Presidential Chair.

It has been a great stimulus to me to work and think. I have known the tense excitement of a great quest, and, at rare intervals, the pure joy of discovery.

If in my two Presidential Addresses I have been able to convey to you some of the pleasure I have found in my work, I shall have repaid some fragment of the debt I owe to the profession which I love, and I shall not have occupied the Presidential Chair in vain.

With these last words, I lay down my office.

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488. [Plato] desires to develop in the human mind the faculty of seeing the universal in all things; until at last the particulars of sense drop away and the universal alone remains. . . . The exercise of the faculty of abstraction apart from facts has enlarged the mind, and played a great part in the education of the human race. Plato appreciated the value of this faculty, and saw it might be quickened by the study of number and relation.—Plato, *Republic*. Jowett, *loc. cit.* p. cix.

489. I hope that posterity will judge me kindly, not only as to the things which I have explained, but also as to those which I have intentionally omitted, so as to leave to others the pleasure of discovery.—Descartes, at close of his *Géométrie*.

INTRODUCTION TO EUCLID'S DEFINITION OF RATIO AND OF EQUAL RATIOS.

By Prof. A. Lodge, M.A.

We start, as Euclid did, with the vague notion that, if A and B are two magnitudes of the same kind, the ratio of A to B is some relation between them in respect of magnitude, *i.e.* in respect of *relative* magnitude, independent of the actual magnitude of each.

Thus if A is twice as large as B, the ratio of A to B is denoted by $A : B = 2 : 1$, and, more generally, if s times A is equal to r times B, the ratio of $A : B = r : s$. In modern notation we can write:

$$\text{if } s \,.\, A = r \,.\, B, \quad \text{i.e. } A = \frac{r}{s} B, \quad \text{then } A : B = \frac{r}{s}.$$

Euclid restricted these statements to cases where r and s were integers, so that $\frac{r}{s}$ is a rational fraction.

It will be seen that this ratio deals only with the relative magnitudes of A, B, not with their actual sizes.

As illustration, take any line LM of any given length and imagine it divided into two parts at an intermediate point K, chosen at random. Now $LK : KM$

may be equal to a rational fraction, or it may not. Let us see the sort of chance that it is. Imagine the line accurately divided into halves, thirds, quarters, fifths, etc., that is, into parts whose ratios can be represented by rational fractions down to, say, so many millionths. This can be done in imagination by drawing lines across LM at the required places. As such lines, being Euclidean lines, are of no breadth, they can be drawn only by the imagination, and moreover, as they have no breadth, they occupy no breadth in the aggregate. Consequently the whole length of LM is still available for more such lines. Consequently it is an infinity to 1 chance against K being *on* one of them. But there must be a great number of cross-lines to the left of K corresponding to ratios which are less than $LK : KM$, and also a great number of cross-lines to the right corresponding to ratios greater than $LK : KM$. That is all we can say about it, if K has been chosen at random; and it will still be the case if we go on increasing the number of cross-lines corresponding to still more rational fractions. The whole area is still available for more, but the compartments are getting rather narrow!

Now take another line $L'M'$, of any length whatever, and put a point K' on it, and take cross-lines again on it, just as we did in the case of LM. Then a certain set of rational fractions will be less than $L'K' : K'M'$, and a certain number will be greater, and its compartment can also be made as narrow as we please, but no cross-line is really likely to actually go through K'. The chances are ∞ to 1 against, as long as we have only a finite number of cross-lines.

Now, the next thing is to see whether if K' and K are placed on their respective lines according to some law, we can devise any test as to the conditions that $LK : KM$ shall be equal to $L'K' : K'M'$.

If we can find that if any crossing line, say corresponding to the rational ratio $\frac{r}{s}$, is to the left of K, so that $\frac{r}{s}$ is less than $LK : KM$, the corresponding line crossing $L'M'$ is also to the left of K', so that $\frac{r}{s}$ is also less than $L'K' : K'M'$, and if also, dealing with those to the right of K, K' respectively,

we can say that if $\frac{r}{s}$ is greater than $LK : KM$, it is also greater than $L'K' : K'M'$, we shall then have located K and K' in *corresponding* compartments, which we can narrow as much as we please. In such a case we can and do say that $LK : KM = L'K' : K'M'$, since no rational fraction can lie between them.

One more step and we shall arrive at Euclid's famous fifth axiom as amended by De Morgan.

The fact is that Euclid had no adequate notation for numerical fractions, and, even if he had, it is doubtful whether he would have used it. He did better by restricting himself to the whole numbers r, s.

Let T be the crossing point corresponding to any rational ratio $\frac{r}{s}$, *i.e.* so that $LT : TM = \frac{r}{s}$.

Then $$s \,.\, LT = r \,.\, TM.$$

Now, if T is to the left of K, it follows that $s \,.\, LK > r \,.\, KM$.

Similarly, if the corresponding point T' is to the left of K', it follows that $s \,.\, L'K' > r \,.\, K'M'$; the opposite inequalities holding when T, T' are to the right of K, K' respectively.

Now we have Euclid's definition of equal ratios as amended by De Morgan.

If A, B be two quantities of the same kind, and C, D be two quantities of the same kind, but not necessarily the same kind as A and B:—

Then if when $s \,.\, A$ is $> r \,.\, B$, $s \,.\, C$ is also $> r \,.\, D$, and, also, when $s \,.\, A < r \,.\, B$, $s \,.\, C < r \,.\, D$, then the ratio $A : B$ is equal to the ratio $C : D$.

Consequences and Extensions.

Thanks to the perfection of Euclid's logic we now know that in considering the question whether two ratios are equal it is sufficient to deal only with the cases in which the ratios are rational, and in many text-books this is done. But we must remember that these proofs are somewhat superficial, and they are safe only because the foundations have been so well and truly laid.

So also in the cognate question as to whether two variable quantities X, Y are proportional, *i.e.* whether $X_1 : X_2 = Y_1 : Y_2$ where X_1, Y_1 and X_2, Y_2 are corresponding values of these quantities, it is only necessary to consider rational values of the ratios. Indeed nowadays we have gone beyond the days in which all ratios are pure numbers, and quite happily work with mixed ratios, *e.g.* to prove that triangles of equal altitude have areas proportional to their bases, all we have to quote is that

$$\text{area of } \triangle = \tfrac{1}{2} \text{ base} \times \text{height};$$

$$\therefore \text{ area} : \text{base} = \tfrac{1}{2} \text{ height, which is constant by the hypothesis.}$$

Practically we can shorten up still further by assuming that two variables are proportional if doubling one doubles the other. These mixed ratio ideas are still novel and to some minds are still "taboo," but there is no doubt that they have come to stay because of their practical value. The argument is that, taking the illustration above, if $X_1 : X_2 = Y_1 : Y_2$ we may deduce the statement that $X_1 : Y_1 = X_2 : Y_2$, *i.e.* $X_1 : Y_1$ is constant. All these things are viewed happily by the mathematical eye, though the logical eye may look rather askance at them.

Though indeed, even from the logical point of view there is not much to complain of. For instance, if A denotes distance travelled, and B the time taken, $A : B$ denotes the speed; and $A : B$ is constant if the speed is constant.

In such a case we do say that A is *proportional* to B, why not therefore speak of their *ratio* and say it is constant? It is a matter of words chiefly.

The main property of mixed ratios is that they introduce new concepts. In this case by comparing distance with time we get the concept of speed,

and similarly from comparing speed with time, acceleration appears, and from weight and acceleration we get the concept of mass, and so on through the whole of mechanics and physics. There is no doubt of the value of considering and permitting mixed ratios.

We are always extending our notions, and often with the happiest results, *e.g.* indices logically are whole numbers, but by extending to fractions, including incommensurables (though we rather slur over these !), we get the whole theory and practice of logarithms. Similarly, factorials expand into gamma functions, and by writing $1+\frac{1}{2}+\frac{1}{3}+\ldots+\frac{1}{x}$ in the form

$$\left(1-\frac{1}{x+1}\right)+\left(\frac{1}{2}-\frac{1}{x+2}\right)+\left(\frac{1}{3}-\frac{1}{x+3}\right)+\ldots \textit{ad inf.},$$

we get a function in which x may be a fraction: hard to explain logically, but all right mathematically. A. LODGE.

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490. To show how giants used to play in Oxford late in the seventeenth century, you might place in your "Gleanings" the New Year's Gift which Wallis sent to Bernard, or *vice versa*, viz. the problem "*Duorum aequalium cuborum alterum sic perforare ut per eum transeat alter integer.*" We have the original in the Bodleian. One cube had to dance clean through the other !—[In a letter from Mr. F. Madan.]

491. Gauss had the right eyebrow raised higher than the left (*more astronomorum*).—M. Wegener. *Biog. Universelle*, 1856, Art. *Gauss*.

492. If p and q are positive uneven prime numbers, p has the same quadratic character with regard to q that q has with regard to p; except when p and q are both of the form $4n+3$, in which case the two characters are always opposite instead of identical. Sect. IV. *Disquisitiones Arithmeticae.* This is the celebrated Fundamental Theorem of Gauss, known also as the Law of Quadratic Reciprocity of Legendre. Gauss used to call this the "gem" of the higher arithmetic.—R. Tucker, "Carl Friedrich Gauss." *Nature*, April 19, 1877, vol. 15, p. 535.

493. He was the first of geometers among poets and the first of poets among geometers.—*Gauss*, of Kaestner, Prof. of Maths. at Gottingen, *loc. cit.* p. 534.

494. One of my most intimate friends in Cambridge, who had been an ardent disciple of the late Sir William Hamilton, Bart., and had adopted the preposterous notions about mathematics inculcated by that master, was consequently in great danger of being plucked. His college tutor took much interest in him, and for a long time gave him private instruction in elementary algebra in addition to the college lectures. After hard labour on the part of each, some success seemed to have been obtained, as my friend had at last for once been enabled to follow the steps of the solution of a question involving a simple equation. A flush of joy mantled his cheek, he felt his degree assured, and he warmly thanked his devoted instructor. Alas, this happy phase had but a brief duration; my friend's early mental bias too soon recovered its sway, and he cried in an agony of doubt and despair, "But what if x should turn out, after all, *not* to be the unknown quantity."—Quoted by P. G. Tait from a pamphlet written by Herbert Spencer. *Nature*, March 31, 1874, vol. 9, p. 402.

495. "The great question is whether my large bookcase will find a place in the inner room. Ever since I have known Cambridge these rooms have been occupied by mathematicians, so that their capacity for library purposes has not been tested, the walls having been used only for pictures and Japanese Crockery."—*Henry Jackson, O.M. A Memoir*, by R. St. John Parry, p. 30. (Extract from a letter to his brother). [Per Prof. H. S. Carslaw.]

THE ORGANIZATION OF SCHOOL MATHEMATICS.

By P. Bolton, M.A., Headmaster, Dean Close School.

When I began the preparation of this paper, I turned up the bulky report of 1912 published by the Board of Education. In the article by Prof. G. H. Hardy I found this passage:

"If only Schoolmasters would devote to the serious study of Mathematics a tithe of the time that they now devote to the organisation of examinations, the framing of schedules, the discussion of pedagogic methods and the writing of inferior textbooks I am sure that the results would soon become visible in our examinations."

I had forgotten this passage; otherwise I should hardly have dared to appear in this room as one of those people whose work is largely concerned with the organisation of teaching and framing of schedules—an occupation apparently on a level with the writing of *inferior* textbooks. I suppose however that, after all, this sort of work must be done. Heads of mathematical departments and even headmasters can find some justification for their existence.

Now this subject, "The Organisation of School Mathematics," is open to more than one method of treatment. It occurred to me that perhaps a comparative study of what is being done in representative schools might be expected somewhat on the lines of Miss Storey's report of some years ago on Girls' Schools. But I came to the conclusion that this was more the task of a committee of inquiry than of one individual. Further, in the report of the Board of Education in 1912 there is available such a mass of information and opinion, and so much pioneer work has been done subsequently in extension and revision by your association, as to make it desirable that the experts actively in charge of mathematical departments should give their views and explain how their organisation of the subject has enabled them to give effect to their ideas.

I therefore shall content myself with certain general aspects which seem to me important in the hope that others, better qualified than I, will supply, among other contributions, some of the details of their own solution of the problems connected with the organisation of School Mathematics.

Now this subject must be considered in relation to the general function of the school, and in assigning to mathematics its place in the curriculum, we have two things to keep in mind, (1) its practical value, (2) its educational value, and we have to consider these in relation to the needs of the boy who eventually specialises in mathematics or allied subjects and of the boy who does not.

I think it is now generally recognised that we cannot make out a very strong case in regard to the utilitarian value of the ordinary school mathematics of the non-specialist. The amount of mathematics that is directly useful in the commerical world is very small and hardly extends beyond the boundaries of the simpler arithmetic, though it has been shown that there is more scope than is generally supposed for the mathematician. What is wanted there is, I think, accuracy, neatness, and quickness, and above all the ability to apply knowledge to practical requirements. I think the school course should certainly supply all that is needed, and I cannot see why there is any necessity for a boy to attend post-school courses of commercial arithmetic and the like. How far we succeed is another matter. To judge from the comments of some business men all is very far from well, and I think it is something of a reproach to schoolmasters that there should be this complaint even if it is only against our dullest pupils. This is something of a digression, but the point I want to make is that we cannot justify the position of mathematics as a common factor in every boy's education merely on grounds of narrow practical utility. We must therefore depend on the plea of its educational value.

In spite of what has been said of late years on this point, I am not sure that, outside the ranks of mathematicians at any rate, the strength of this plea is recognised. I think perhaps we are ready to put forward the claims of mathematics based on its value as a subject requiring logical thought, and one moreover in which the student can measure his success to a large extent. We are ready, too, to point out its worth in developing intuition, imagination, and the faculties of deduction and induction. But we are not always quite so alert to emphasise other aspects such as those so fully and ably discussed in the appendix to the Board of Education report on the teaching of English. The concluding paragraph will bear repetition; it runs: "While therefore Mathematics is of all subjects the easiest in essence, it is also the most exacting; ignorance and obscurity of thought cannot here be covered by a cloud of words however beautiful." Anyone who has taught physics will appreciate this particularly, for it is illuminating to note how the question which requires a substratum of mathematics for a really satisfactory elucidation sorts out the boy of real understanding from his neighbour, who does so frequently rely on a cloud of words to conceal his ignorance, or perhaps because he does not realise his ignorance.

The quotation goes on: "If this essential characteristic of Mathematics is ignored it is difficult to see what claim it has to be regarded as a necessary element in the education of every individual." All this is, of course, in connection with the training which mathematics provides in the use of speech. While I am not prepared to go quite so far as the writer of this article, I do believe that he gives strong additional justification for the inclusion of mathematics for all, especially now that Latin has to some extent lost its pride of place as the Highest Common Factor in our educational system.

I emphasise these views of the general educational value of mathematics because I feel that, at least from the headmasters' point of view, it does very definitely necessitate that the organisation of the subject shall be such as to provide primarily for the non-specialist. His needs must have first claim. In actual fact I do not think that the application of modern ideas is detrimental—distinctly the reverse—to the development of the specialist. But if any clash of interests were to arise the specialist would have to give way.

The changes of the last fifteen years have enormously increased the efficiency, the ratio of the good to the total, of school mathematics, particularly for the non-specialist, and this is all to the good. A great deal of old-fashioned work, tedious, artificial and uninspiring, has been cut out—officially at any rate, though I think it survives in many backwaters. I fear, however, that in spite of many recommendations there has not been a commensurate advance beyond the old barriers, and this is very unfortunate. I believe there is a large number of boys who never get beyond the School Certificate syllabus in arithmetic, algebra and geometry, and are never even introduced to the elementary principles of mechanics or trigonometry, much less of calculus. The reports of the Oxford and Cambridge Board, for example, show that in their School Certificate, additional mathematics is taken by only about 25 per cent. of the candidates and passed to credit standard by only about 10 per cent. In the Higher Certificate, excluding the Science and Mathematical Group candidates, this last year there were only about 40 passes in subsidiary mathematics out of the 800 who passed in Groups 1 and 2, and some of those no doubt took only the arithmetic, algebra, geometry paper.

I do not want to infer too much from figures of this kind. Work is done that is not always tested by these examinations, and moreover the Joint Board deals very largely with a particular class of schools, but it seems to suggest grounds for the suspicion that the mathematics of the general educational course is still very largely comprised within the boundaries of arithmetic, algebra, and geometry. I am not satisfied that this limited range enables us to get the full educational value of the subject. I should like to see the basis

of the subject broadened in accordance with the recommendations that have been made so frequently. I should say that we give a liberal measure of time to mathematics during the greater part of a boy's school career, and I think that we ought to be able to do more than at present to introduce the ordinary boy to the elements of trigonometry, mechanics, calculus, etc. But it goes without saying that the organisation must be such as to provide for treatment of these subjects on the right lines, which implies, for example, arrangements for field and laboratory work. There is no point in replacing the bath problems which used to invite our scorn by the elaborate identities of trigonometry, or the dinner-table permutations problem of algebra by those in mechanics which are concerned with the billiard cue resting in a flower pot, or the height to which a bricklayer must climb his ladder in order to make it slip. I know that we repudiate that type of problem, but I believe the textbooks which are responsible for it still find a ready sale.

Now, though I am emphatic that the organisation of school mathematics should care especially for the non-specialist, I do not think that the specialist need suffer. I think that all the traditional work that is really valuable, and which does not properly form a part of the general course, can be done at the beginning of the specialisation period, when it will come far more easily to the boy whose outlook has been broadened by a more extended course. Nothing will do away with the need of the best possible specialised training for those who are capable of profiting by it, and due provision must be made for it, but in my experience it is a simpler problem than that of providing, for the non-specialist, a coherent scheme which really fulfils its educational purpose.

Then there is the sub-average boy, the so-called dull boy. I believe that all schools have a very definite responsibility to this class. I am not satisfied that we are justified in applying hard and fast examination tests for entrance to schools and even to universities, or that superannuation schemes and the like can really be justified. They presuppose that all is well with the school curriculum, and leave out of account all responsibility for shaping it to supply the needs of the dull boy. I feel very strongly that all schools have a definite obligation here, and I believe that, particularly on the practical side of the extended mathematical syllabus, there is scope for the development of talent which, in more cases than are realised, merely lies dormant. Further, I know from experience that when such talent is awakened and developed along what you may call, if you like, these side-lines of mathematics, the effect is far-reaching and may indeed vitalise the whole of a boy's work.

This implies a further requirement of the mathematical organisation. It must be elastic; work must be guided, not fettered by syllabuses. So we have three classes to provide for in our organisation of school mathematics.

The problem is to a great extent simplified by the general acceptance of the school certificate as the first goal and the fact that most pupils remain at school at least long enough to work towards that objective. I know there are objections to these examinations, that they exercise an undue domination over us, but the remedy is in our own hands. At any rate there is a certain definiteness and unity of purpose about this stage, and a boy's general line of work can be fairly clearly defined. It is therefore possible to adopt any convenient classification into forms in accordance with general capacity in, say, English subjects and to reclassify groups of forms into sets for mathematics. In this way, which I suppose is very general, there is provided the necessary elasticity which enables every boy to progress at his own best rate. Difficulties may arise at the transition stage from one block of forms to another in the case of boys whose progress in form group subjects is very different from that in mathematics, but that I think is unavoidable under any scheme, and it is of less importance where the end in view is the general intellectual growth of the boy rather than his acquisition of so much information on this or that subject.

But elasticity must be accompanied by the closest co-operation between teachers of different subjects, and the mathematical organisation must dovetail into that of other subjects. Now I have no faith in any form of correlation which is in any way artificial or forced, but there are obvious opportunities of close and effective union between mathematics and, for example, physics. I think it should be axiomatic that some masters, if not all, should be teaching both mathematics and physics. To give effect to this requires the aid of the headmaster, as it involves problems of school, as distinct from subject, organisation, but both mathematical and scientific associations have spoken so forcefully on this point as to leave the non-mathematical or non-scientific headmaster in no doubt as to the opinion of the experts. The report of the Prime Minister's Committee of 1918 on the position of natural science in our educational system lays great stress on this. It points out the gain both to physics and to mathematics which results from having the teaching largely in the hands of the same men. But its next point is of special importance; it runs: "Any want of co-ordination between Science and Mathematics has a particularly unfortunate effect on the teaching of Mechanics. The subject is rightly regarded as of the greatest importance. It is the basis of most parts of Physics and most schools make some provision for teaching it either as a branch of Mathematics or as an experimental subject or from both points of view; but the evidence we have received indicates that in a considerable number of schools the results obtained are far from satisfactory. This is much to be regretted, for the subject is specially suitable for training the student to use his Mathematics and to apply it to the problems which he may have to face. The power of doing this is a very valuable asset for those who will become engineers or be occupied with other applications of Science to industry, while, apart from this aspect, it affords a mental training of the highest importance and gives to many an interest in Mathematics which, without it, they would never acquire."

I am thoroughly convinced of the value of combining the two subjects. My own experience is that a master with a really effective knowledge of both is capable of giving to his mathematical teaching far greater vitality than is ordinarily the case. And, again, the teaching of physics is leavened with the greater precision of the mathematician, and does not degenerate into the mainly descriptive science which it may well become in the hands of some teachers who by training and interest incline away from the mathematical side of physics.

I do not think that in general the school organisation required to give effect to these principles is difficult even in the smaller schools, where of course power of adjustment is limited by the size of the staff. In fact it may well result in increased adaptability, for very often the science masters are occupied fully in their own subjects, with the result that *their* engagements govern the timetable of a considerable portion of the school. In such cases there may be great relief by the interchange of work between physicist and mathematician. A more serious difficulty, however, is the supply of teachers with the necessary qualifications. There has been a great increase of interest taken by mathematicians in experimental science, but I do not think there is yet anything like a large enough number of men who have done a full three years' mathematical course and followed it up by a fourth year of science. Here I have in mind particularly the supply of teachers who are primarily mathematicians capable of doing the most advanced mathematics. I think it is generally admitted that it is easier to get the natural science master who has reasonable qualification for the teaching of elementary mathematics. We do want the mathematical expert, but I consider that he should have a broader outlook than the purely mathematical course at the university has provided in the past.

Another subject which must be taken into account in the organisation of the mathematical teaching is geography. As you know, this is a subject which

is gaining increasing recognition—it is even now invading the Higher Certificate stronghold of the Oxford and Cambridge Board. It gives an excellent opportunity of providing mathematics with a background of reality which is invaluable. This has been pointed out again and again, but, so far, I cannot see much more than an indication of the interaction of the two subjects. Here again we are dependent on the supply of teachers with the right training, and I believe the institution of the Geography Tripos at Cambridge, with the recommendation that men should read Part I. of the Mathematical Tripos, is doing something to produce the right type of teacher. A master with the outlook which such a training should give cannot fail to appreciate the value both to mathematics and to geography of practical work in the field, surveying, measuring, making plans. He will want to introduce numerical trigonometry, solid geometry, etc. He will need no pressure to work on the lines which have been laid down by your association. The organisation of school mathematics must be such as to give him, too, his opportunity of developing his full usefulness.

To make the most of these reforms something more than class rooms is needed. It would be a great thing if we could afford to scrap the mathematical class-room with its rows of desks and replace it by the laboratory. Some schools are fortunate enough to have such facilities, but in many the physical laboratory is still the only room of the right sort which is available.

There is also the question of equipment and maintenance. But these difficulties can be surmounted—apparatus can be made in the school workshops, and in that way it is twice blessed. Moreover it is possible to go beyond the limits of the usual school sets of the simplest appliances and incidentally provide work which I believe will be generally welcomed by the workshop staff. Apparatus can be set up in a more or less permanent way round the walls of the room, so that it is quickly available and needs little labour of setting up and storing away.

Such a room becomes at once a drawing office to which the problems of the workshop can be brought. It affords the best of facilities for the working out of extended problems by groups or teams of boys. It is in every way adaptable, and to my mind is so attractive as to be almost a source of inspiration in itself.

These are some of the considerations which have impressed me as important in the organisation of the mathematical teaching in schools. They have been emphasised many times in the various reports and detailed schemes which your association has produced, but I do not think we have yet translated them into practice as fully as we should. Perhaps I have taken up time which ought to have been given to a more detailed discussion of the immediate problem. If so, I hope that others who are concerned with the organisation of school mathematics will make good the deficiency. I have been mainly concerned with the subject as part of the question of the general educational system, and that for two reasons:

First, the broadening of the basis of the subject gives the non-specialist a more vital interest in it, and in particular may open to the dull boy an avenue of intellectual advance which is often denied him in other subjects; and

Secondly, while I think it lays better foundations on which the later work of the specialist can be built, it secures the place of mathematics in an educational system which is laying increasing stress on the real function of education and is examining critically the claims of new subjects for a place in that system.

If we are satisfied that mathematics has a valuable, perhaps unique contribution to make, we must, to quote Professor Nunn, learn to question accepted values and to inquire in a critical spirit what parts of the traditional curriculum are really vital and what parts have only a conventional value.

The **Chairman**: Before asking you to join in a discussion on this very important subject of the organisation of school mathematics, I would like to say that Mr. Bolton has spoken specially about the organisation of mathematics for non-specialists among boys, and I hope there will be some report

as to what is considered best in the case of girls in this subject, because they also want trigonometry as well as the ordinary algebra. It is evident that Mr. Bolton, as a headmaster, is favourable to a good mathematical course. I remember a certain boy who was said to be specially good in mathematics coming in as an entrance scholar, and we wanted him to go to a high place. But the headmaster said: "Good at mathematics? Is that to his credit, or the reverse?" (Laughter.) That seems a bad beginning for the chances of a boy. It was perhaps a mistake that the boy had specialised too early. This question of over-early specialisation is one of those that we have to consider.

I think, now, before asking for the general discussion, it will be well to ask Mr. Siddons to give his paper, because examining school mathematics is a related and important subject.

THE BEST METHOD OF EXAMINING SCHOOL MATHEMATICS, WITH SPECIAL REFERENCE TO THE SCHOOL CERTIFICATE EXAMINATION OF THE OXFORD AND CAMBRIDGE SCHOOLS EXAMINATION BOARD.

BY A. W. SIDDONS, M.A.

LADIES AND GENTLEMEN,—Before this meeting commenced, Professor Lodge asked me if I was doing much examining now, to which I answered "No, none." Then he asked what qualification I thought I had got for speaking on the best method of examining. I do not want to speak much on the best method, but I think it is time that members of this Association had the opportunity of blowing off a lot of steam, and I hope that many will have something to say about the school certificate examinations. My desire is that many different people should give their views and their criticisms.

I shall deal with outside examinations entirely. And, first of all, what is the schoolmaster's test of a good examination? It is whether his own boys get through. (Laughter.) But I do not want to be foolish in the matter. Many schoolmasters will tell you, honestly, that certain boys get through the certificate examination and yet they had no business to do so. When you get a man in his honest mood he judges an examination not by whether or not his own boys get through, but whether the right boys get through. In certain certificate examinations that has not always been the case: it has not always been the right boy who got through.

I think the first charge on the examiner should be that he should set a paper which gets the right boy through. And the examiner must also consider another point, and that is, any possible influence on the teaching. My own opinion is that, if possible, the school should entirely ignore the examination. You may, I think, divide candidates into three classes: (1) those who are certain to pass, whatever the examination is; (2) those who are certain to fail; (3) an intermediate class of which one does not know what will happen to them. It is this last class which, I think, the examiner should consider more carefully. I do not care about the top people. If I send in a dozen or twenty boys for a school certificate examination, I know, better than the examiner does, which boy has mathematical ability and which has not. But the examination is not designed to show that, it is to show which boys have reached a respectable standard and which have not. I have never known cases of boys good enough to ignore the certificate examination entirely who have failed in that examination. What we want to keep our minds on more are the pupils near the borderline. Of course I am considering only "Pass" examinations.

The first question I want to put is, what type of paper will secure that the right boy gets through? Mainly I can speak simply from internal examinations, that is to say, examinations of our own, though I have had some experience of outside examining as well. Still, I shall give my opinion mainly from the inside.

I am convinced that if you set easy papers you get far more reliable results as to boys' relative merits than you do if you set hard papers. I am equally certain that, looking at results in certain years in which the paper has been easy, the results were much more to be trusted than those in the years in which the papers were hard. Another thing is, I think it is very much better to have mixed papers rather than separate papers on arithmetic, algebra, and geometry. The London Matriculation examination takes algebra and arithmetic together, and geometry in a separate paper. The Oxford and Cambridge school certificate examination takes all three subjects in each paper. As a safeguard, the Oxford and Cambridge Joint Board insist on boys doing the first three questions in each paper, and then selecting after they have dealt with those. The first three consist of an algebraic question, an arithmetic question, and a geometry question. So in that, I think, there is some safeguard that they are not neglecting one subject entirely for the others. But the reasons that I think the joint papers are better is that, I believe, it produces a better type of question; it keeps us away from the stereotyped textbook question. I have been teaching now for nearly thirty years, and I still remember the day when it was a question whether a boy was allowed to use x in an arithmetic paper. Of course you smile at that now, but it was a very real difficulty twenty-five years ago. When I first began talking to this Association it was something we had to fight for. The mixed paper has tended to produce better questions and to vary the subjects more as well as to make the examination more real. That is why I like mixed papers much better.

If there are examiners here to-day—and I hope there are some—I want to plead very much for the boys who belong to the intermediate class, boys in regard to whom you do not know what they will do. It is astonishing how those boys are put off by a bad question. Algebra is the subject in regard to which the examiner sins worst. Look at this question.

$$\text{Simplify: } \frac{1}{x^2+a^2-b^2}+\frac{1}{x^2-a^2+b^2}+\frac{1}{x^2-a^2-b^2}, \text{ where } x=a+b.$$

There is a certain symmetry about that question: there are plus and minus signs scattered about pleasantly in it. A colleague of mine who invigilated in the paper told me it was pathetic to see the time which was wasted over this question. And what interest is there in it? It is purely testing technical skill. That question put off many boys who were borderline candidates. Perhaps it ought not to have put a boy off, but it did. That question, I think you will agree, was not a good one.

Another question was this:

Draw a line $AXBY$ with $AX=1''$, $XB=\frac{1}{2}''$, $BY=1\frac{1}{2}''$; if P is a point such that $PX=2''$, $PY=4''$, prove that the angle XPY is a right angle.

The student was asked to give a graphic demonstration. But there is no triangle with such sides! (Laughter.) It is unfair and untrue, and no practical body and no schoolmaster with a head on his shoulders would have allowed such a question to pass into a paper.

I should like to lay down an axiom—an axiom, I was once told, was something which was known to be true though it might not be—(Laughter)—and I would like to lay down an axiom for guidance, namely, that every examining body for anything like a school certificate should have either a practical schoolmaster, or an ex-schoolmaster, engaged in the setting of the paper, or at least to be required to look at it before it goes to the candidates. In fact you might say they should have a moderator. Some time ago I protested against a certain paper and said I was sure it had not been set by a man who had had practical experience of teaching mathematics in schools. I was informed that the criticism was particularly unfortunate, and was told who the examiners were. One was a science master who had never been a mathematical master, one was an Oxford don—I meant to say a don; I did not intend to mention

Oxford—I want to leave it as obscure as I can. The other was a university don who had taught in a school during the War. Do you think those types of people are the people who really know what we want in examining mathematics in schools? I do not think they are. I have known many mathematical schoolmasters who are incapable of really understanding the ordinary boy's difficulties, because they were too good mathematicians themselves to get down to the level of the comparatively stupid pupil. A don is probably far too clever to understand the average boy. I think the examining body should have a schoolmaster or schoolmistress who is either actively engaged in teaching, or who has been comparatively lately concerned with teaching, because it is astonishing how the weak borderline candidate is shaken by a question which is not a fair question, or by a question which is not only not fair but which sends him off on to the wrong track. I refer to such questions as those in which the candidate gets lost in a mass of figures. In regard to the algebra question which I first put up, the boy who has got good mathematical ability will look at it, and he will know when to give it up; but another will go on and get lost in figures, and will waste an extraordinary amount of time. One of the worst questions I have come across in an examination was this—I cannot quote it exactly—

Evaluate $23{\cdot}72^3 + 0{\cdot}5362^2$ by means of logarithms (four figure tables were supplied).

We were asked to calculate, by logarithms, the value of that. The examiner had to supply answers to the questions he set. He gave eight significant figures, and he got four from the first term and four from the second. I jumped on that at the examiners' meeting, and the question did not go forward to be set. No practical schoolmaster would pass it.

I have referred to some bad questions; here is another:

At the Oxford and Cambridge school certificate examination they asked for proof of Euclid, I. 4. That theorem is mentioned in their syllabus, and it is marked with a sign which means that proof of that theorem will not be asked for. If you had had a man acting as a moderator to that paper who had taken the slightest interest in the discussions which have been going on in the last few years on the teaching of geometry, the moment he saw that he would have looked at the syllabus and condemned it. You may tell your boys to ignore the question, or you may take no notice of it, but it is not right to set it; it shows carelessness, and it is not fair, particularly to the borderline candidates; it does not matter so much to the upper or to the lower candidates.

I have worked out some statistics lately, which, however, I do not propose to inflict upon you. I worked out the percentages of credits obtained in the Oxford and Cambridge school certificate examination in recent years. (Table exhibited.) The table shows that the percentage of credits fluctuates a great deal. In mathematics we should be able to keep the standard more easily than in any other subject. But in 6,000 candidates from the same schools every year I do not think you are likely to get much fluctuation of standard. There may be a steady drift of standard, but not sudden ups and downs like 53 per cent. of credits to 43 per cent.

On looking at a recent paper on one occasion I said "We shall be badly hit by this paper." When we came to receive the marks I found they had reduced their credit mark from 175 out of 300 to 165, and their pass mark from 145 to 130, thus confessing that the paper had been too hard. And they reduced the number of credits from 53 per cent. to 43 per cent.

In many schools mathematics is the one subject which is compulsory: if for the certificate a candidate must reach a certain standard in the mathematics and science group, it usually means he must reach a certain number of marks in mathematics. This places mathematics in rather a peculiar position, and if mathematical examiners do not take more pains about it, there will be trouble brewing, because I think that, at the moment, the pen-

dulum is swinging in favour of the classics. A few years ago the boys seemed all off Latin and Greek, but the tendency to-day seems for them to be drifting back to those subjects. Already classical people are talking about boys who are getting so many credits in other subjects, but they fail in their certificates because they do not reach the necessary standard in mathematics. If that kind of thing goes on, we shall get influential headmasters beginning to persuade the powers that be to let five or six credits in Groups 1 and 2 compensate for not reaching a sufficiently high standard in Group 3, mathematics and science. And I am not sure they would not be right. But we should watch that.

My own feeling over the examination is that there is not a sufficiently wide gap between the credit mark and the pass mark. It would be a great help if we were to lower the standard a little for the weaker candidates in mathematics without lowering the standard of the credit.

There is another thing which I noticed about the additional papers: that often there is a tendency to set the hard question of the paper at the end. I think that is a pity; I believe it is better to set the hard question earlier. And there is a tendency, in the additional mathematics, to get harder and harder, while there is a growing trend towards taking the school certificate earlier. And the result of that is that fewer boys are likely to be taking additional mathematics. I notice that the classical specialists begin specialising and getting their certificates earlier than they used to, and they go out into the world with fewer of them having a knowledge of trigonometry and mechanics and the calculus, of which I think everybody with brains should have some knowledge before he goes out into the world.

There are probably many here who have grievances and want to "blow off" about these examinations, and so I propose to say only one thing more. We do not want examiners to be looking out merely for technical skill. It is so easy to take fractions in algebra and keep people down to such questions; but technical skill is not what is wanted. We want to give boys ideas, and examiners should be looking out for ideas. But it is difficult to be examined on ideas. Our examinations are not framed for the tip-top people, nor for the very bad ones at the bottom; they are concerned with the intermediate people. For that reason the examination should be more straightforward, and, at any rate, the questions should be set with care, and we should not have bad questions set which cannot be worked, or questions which are outside the syllabus.

I hope we shall have a considerable discussion on the matter.

Mr. **A. J. Taylor:** With regard to the first paper, there is one suggestion I would like to make. Mr. Bolton told us that the great problem of organisation is the non-specialist. I suppose all of us have felt that a large number of our boys leave school without knowing what mathematics is at all. Might I suggest that a way out of that difficulty is the teaching of mechanics? I think a general course of mechanics, treated from the mathematical rather than the physical point of view, might include both the elements of trigonometry and the elements of the calculus to such an extent that a boy leaving school after such a course would at least know what trigonometry was about, he would know what was essential about it—and there is not much in trigonometry which is essential—and he would know something about mathematics. I do not mean that a very elaborate course of mechanics should be followed; it should not involve very complicated apparatus, nor should it involve individual work in the laboratory. I mean, rather, that it should be a course in original mathematics, in which the practical part was merely suggestive; and perhaps now and again verification and deduction made from ordinary experiences, for boys have a good deal of experience of mechanical things in these days.

When we come to the higher schools, I think it is absolutely essential that one man should take both the pure and the applied mathematics. I do not know what the rule in schools is. My own experience is unfortunate, for I have to take the applied without having to do with the pure, and there are many difficulties that I have to encounter. You want the boy to have done something in the calculus in the "pure" class, and you find in the practical class that you want a symbol from him with which he is unfamiliar. One instance is in differentiating the sine. The boy in the "pure" class says cosine x, no emphasis being laid in the pure class on the fact that it all depends on what the x is.

Mechanics is a subject which is grossly neglected in some schools, from what I can gather, and yet it seems a way out, both for the non-specialist and, later, for the specialist, in that you can weave together so well the two subjects, mathematics and mechanics.

With regard to examinations, there is only one thing I want to say. Mr. Siddons told us he thought the best way to examine the middle set of boys was to set them easy questions. I wonder how far this is due to the fact that easy questions, or short questions, can be easily marked. You are probably all familiar with the question papers which have been sent to various examiners to receive their markings, and the great differences in the range of such markings. There is a case quoted in which a geometry paper was sent to over 100 different examiners, and a frequency curve of their markings was given. The marks ranged from 35 per cent. to 85 per cent. That is a remarkable thing for a geometry paper.

Now, taking the questions which were put up on the board, in regard to the algebra question I wondered what would have been the result of splitting that up into little pieces, taking each denominator separately, and having substitution done there. What difference would have resulted in regard to marks in the case of the weaker boys? I do not know why boys are weak in examination in this respect in hard questions, but they seem to me to be lacking the power of dealing with a complex whole. I do not know whether that is all the difficulty; if it is, it does not seem a sufficient cause for their failure. Many of these weaker people are plodders; if they get hold of a question they will not leave it until they get to the end—which, often, they do not in the time allowed—and they have fruitlessly used up very valuable time. I have seen a boy spend an hour over a graph. In the second place, you may get weak boys who just never take hold of the question, but dodge all over the place. If we had more questions, and shorter ones, with a few more complex questions for the better boys, we should avoid many of the difficulties which we at present encounter in our examinational system.

A **Member:** Mr. Siddons said he wants the "right boy" to get through in the middle lot. Which is the right boy? Until you know what you have to test, you cannot test the boys properly. If you examine these middle boys with a large number of easy questions, say, in arithmetic, you will probably get a rather different order from that which you will if you examine them with a few longer questions involving a certain amount of reasoning. Which are the boys Mr. Siddons wants to get through?

The **Chairman:** I wanted to ask the same question. Mr. Siddons wanted easy questions, and yet questions involving a certain amount of thought. It seems to me they are in two different categories altogether. A criticism I had once with regard to an arithmetic paper, from the Headmistress' Association, was that there were not enough routine questions to enable girls to get through without individual thought—(Laughter)—whereas from a boys' school, with regard to the same paper, I got from the headmaster a statement that the boys enjoyed the questions, which made them think, and that is what they wanted. And that raises also a very important question, which I touched on just now: Do girls want the same standard in mathematics as boys do for

passing? Or ought we to relax them a little for girls. If we take them through the same public examinations there will always be a pull down on the girls' side. If we make a lower standard and make it a more routine matter, we shall spoil our examinations. I should like this to be in the minds of those who join in the discussion.

Miss **Waters**: I have had experience in both capacities, as I was a teacher for over twenty years, and I have been examining for twelve years.

With regard to organising, I would like to say that I have had experience, as a headmistress, of how difficult it is to get mathematics organised in any way satisfactorily. Of course, in girls' schools we are specially handicapped in that we do not consider ourselves justified in giving the time to mathematics that is devoted to it in boys' schools; therefore if we have to pull the girls up to the same standard, we are even more impelled to resort to cramming than are the teachers of boys. Personally, I would like to scrap three-fourths of the contents of the algebra books and half the contents of the geometry books, and in this way get rid of an enormous amount of dead matter. We are dominated by the university girl or boy. Though only a very small percentage of those in our secondary schools pass to the university, the curriculum in mathematics is designed for them. And our examinations are dominated largely by the university. Most of the examiners are university men, men who have not been associated with school difficulties, but who are tremendously sympathetic in regard to them; you cannot sit at a meeting with them without noting the minute care they give to questions, but they are men who have never taught at all children under fifteen years of age. We must remember that their ideals in mathematics are rather different from those of the schools. We want to turn out examination papers for the average man and woman who will go out into the world, and if that is kept in mind we shall get rid of an enormous mass of material which is demanded from those who pass into the university. Only in that way shall we do something to separate the demands of the university from the demands of the pass certificate. You cannot prepare a boy or girl for life and at the same time prepare them for a series of years at the university in any given subject. What I want for girls, in any school, is everything in mathematics which is worth teaching, and on much of that you cannot examine. We want mathematics to give them ideas, and you cannot examine on ideas; the candidates cannot even reproduce them, much less think them out for themselves. We produce nice little technical sums, such as Mr. Siddons has caricatured this afternoon, and we cannot help ourselves. Or you have some of these good ladies saying there is not enough for the girl to do of the same type as she did last week in class. You can teach a girl a lot of interesting mathematics, and at the end of the year she cannot give you the factors of a simple expression, but she has got the ideas; only you cannot prepare her for the school certificate at the same time. We want to get a mathematical scheme which will prepare the pupil for life, and I would emphasise that we want very much more of the practical side in girls' schools and much less pure mathematics; more applied mathematics is required. But, for this, we shall have to find the teachers; the number of women about who are professing to teach mathematics but who would be tied up with an ordinary problem in applied mathematics makes one shudder.

The shorter question type of paper is good in its way, but it is easily crammed for. In three years' time all the types will have been exhausted by the examiners, and all teachers will have crammed them into their pupils. What is the good of that? It gets them through, but what is the real benefit? We dare not consider now the morality of cramming for examinations, for the child's whole future is at stake.

I would like to give my experience as an examiner. My experience is that it is very difficult to get such changes as Mr. Siddons spoke about. There are

moderators; sometimes I wish there were not, for if you try to do something original they have a way of turning it down. I once had an arithmetic question refused on the ground that it seemed to be common sense more than arithmetic. I did want to retort that the two *might* be divorced, but I had never heard of it.

I know it is felt by men that girls are weak in mathematics, and it is true. I do not believe there is any congenital difference in capacity between the two sexes, but what with the smaller scope and the smaller number of competent women teachers of the subject, I am afraid that the number of good mathematical women about is small. It is difficult to get a really competent mathematical mistress. I wonder whether the problem could be solved by keeping mathematics on its present lines as a necessity for matriculation for university people, and having much easier papers in it for the pass certificate. Or we might be allowed to take an alternative, having an extra group with some other subject in it, instead of forcing mathematics as an essential. I would personally say that if you want to teach a subject well, do not ask anybody to examine it.

Mr. **Thompson** (Oxford and Cambridge Board): I have had many years' experience as an examiner and as a member of the Board, and it seems to me that Mr. Siddons has picked out a few questions in which undoubtedly there have been mistakes; but I wonder how many examinations of the same magnitude and the same kind would yield fewer mistakes if carefully looked into. It is, of course, deplorable that these mistakes should happen; they ought not to happen. But I am bound to say it is not from want of care on the part of this particular examining body. The papers are set by Boards of Examiners, they are sent to Revisers, and finally they are tested by an independent person.

Mr. Siddons said that every board of examiners ought to have a schoolmaster as a member, or at any rate a moderator. Many schoolmasters know who compose the examining boards, and they have a pretty good opportunity of judging who are examining. For quite ten years there has been at least one ex-schoolmaster on the Board of Examiners for the school certificate, perhaps for fourteen years.

I regret deeply the loss of Charles Godfrey and that he cannot be with us to-day; he had much to do with the organisation of the examination as it now stands; he was one of the Board of Examiners for many years. His place has been taken by another distinguished ex-schoolmaster. Therefore these mistakes happen, in spite of having able and experienced schoolmasters on the Board. I do not think it is more likely that a schoolmaster would discover mistakes, but I admit that one mistake quoted was a thoroughly bad one. (The **Chairman**: You don't discover it until you write it down.) No, you don't see it until you try to work it. I do not know who set it, but it was passed by an ex-schoolmaster.

The question of the average boy has been raised, and that of the boy who is on the borderline. I have had many years' experience, and I say that examiners take a great deal of interest in the borderline boy. I defy anybody to set papers over a series of years which shall be absolutely uniform, and if you set for years papers of the same standard, how dull they will be! If you vary the questions, you are sure to catch some boy out. You try to think of something new, and if you succeed in this, it is always said that it is too hard and that it leads to deplorable results. At any rate, examining bodies are composed of men like yourselves; and perhaps many of you here are examiners. Whenever we find we have made a mistake, we take every possible care to make allowance for that mistake; there are some allowances which we cannot make, but we make a good many.

The question has been raised of the number of passes in Christmas examination in comparison with that in the summer examination. I have not the

figures with me here, but the number who take the summer examination is far larger than that of the winter examination, and has increased considerably. Therefore you will find a more average uniform standard in the summer. I have always understood—and I do not think Mr. Siddons really contradicted it—that those who take the certificate examination in the winter are to a considerable extent the weaker boys, those who failed at the summer examination. The standard varies every time, and if you are going to suppose that candidates will be uniform, I think your results will be less satisfactory than they are now sometimes alleged to be. Every good board of examiners makes allowances and considers candidates as a whole. The examiner sees how the candidates are doing, and if he finds that a boy's work seems rather good but that he gets only low marks, he begins to think the paper has been rather hard and he makes allowances. I call that right examining.

Mr. Siddons said that technical skill is not wanted, but there must be some amount of technical skill; you cannot expect people to do a paper unless they can use symbols intelligently.

Allusion has been made to the earlier age at which school examinations are often taken nowadays. There is always some pressure being put on the examining body—certainly on the Joint Board—to enable a boy to take the examination at some age other than the recognised age for the examination. We have followed the Board of Education scheme: first the examination of the school certificate type, and, two years after that, the further examination of the higher certificate type. If boys come in for the school certificate too early, or for the higher certificate too early, there will be trouble, and it is not fair to blame examining bodies if boys are put in for certificates at ages earlier than those contemplated in the regulations.

We deplore the mistakes that occur; I do not seek to minimise them. But I do not think they bear any appreciable proportion to the whole of the questions set in the examinations. Every care is exercised to avoid mistakes creeping in. To say that a super-schoolmaster will prevent all mistakes—well, I do not think he will.

Mr. **Siddons:** I would like to ask the last speaker a question—if I am rightly informed, that the particular schoolmaster who has been examining for the last five years is a man who has not taught mathematics in a school? He took a mathematical degree, and taught another subject in school, not mathematics?

(Mr. Siddons and Mr. Thompson conferred.)

Mr. **Siddons:** I am wrong.

Mr. **Katz** (Croydon): Following upon the remarks of Miss Waters, I think we have to revise our ideas; at any rate, we must criticise the ideas we have as to why we think mathematics should be taught at all to the non-specialist, and I am excluding not only mathematical specialists but also engineers and actuaries, who make considerable use of mathematics. I think it is admitted that the proportion of our pupils who will not use their mathematics is about 90 per cent. Therefore the question arises—Why should we teach mathematics at all? One reason which in the past has been given is that you should teach every boy at least the arithmetic or the mathematics of citizenship. But when you go into that you find that if you know enough arithmetic to count your change, and to read off the numbers of the houses in the street, you are in possession of enough arithmetic and mathematics to get on with. It is about all the effective mathematics which 90 per cent. of people are called upon to use. If they want to work out their income-tax, they go to experts to do it for them. And there are now calculating machines and all sorts of specialist devices to get over the fact that in our complex civilisation some mathematics must be used. So that explanation fails.

The more popular one is that mathematics should be taken by non-specialists

because of its mental training. If we are honest in our ideas, we have to admit that the value of this mental training has been enormously exaggerated. If we are candid with ourselves and look around on our mathematical friends and ask ourselves whether they are men of remarkable interests and gifts, we have to admit our doubts. It has been said that mathematics gives a training in logic. Yes, in logic of a very special kind, mathematical logic. As a matter of fact, most of the thinking we do in real life is not of the mathematical type. All we can say is that if you do more mathematics of the severely logical type, it will increase your competence to do still more mathematics of that type.

Assuming that these ancient grounds for teaching mathematics break down, why then should we teach mathematics? We have to say that the real reason is the cultural reason: the child should learn mathematics because, better than most subjects, it will enable him to discover what a wonderful thing his mind is. I take the view that that is the strongest reason of all why the subject should be taught to non-specialists.

I agree with Mr. Bolton that we should widen the scope of mathematics for the non-specialist. Mr. Bolton has urged the importance of including mechanics and trigonometry, and I would go further and say we should do everything possible to include not only a certain amount of calculus—anyhow the beginnings of the calculus—but also a subject like elementary astronomy. I hold the view—and probably many will agree with me—that the mental stimulus to be derived from a study of the beginnings of astronomy is greater than that derived from studying physics and chemistry. But mentally stimulating material is not the sort of thing that can easily be examined. I will not go so far as to say we should not have examinations at all; but some of the best things the non-specialist derives from mathematics are not such as come out in the examination.

And that brings me to the other point I want to make. I agree with what Mr. Siddons said, and his speech raises this question: Seeing that so many boys derive benefit from mathematics, why are there so many bad examinees? When they enter the examination room their knowledge collapses, or is not of the sort which can be easily examined. If that is the case, should not we, as the Mathematical Association, frankly advocate the abolition of regulations making mathematics compulsory on every boy who takes the school leaving certificate? Should we not say, we don't want this subject rammed down his throat? I leave open the question whether every boy should learn mathematics. That is a wider question. Even if every boy does a certain amount, why insist that every boy when leaving should be submitted to a test? The Headmistresses have sent a memorandum to the Board in which they advocate a rearrangement of the syllabus for the school leaving examination, which will make it possible for the examinee to leave out not only mathematics and science, but arithmetic also. And it is interesting to note that while the Headmistresses were in favour of the abandonment of arithmetic as a compulsory subject for the school leaving certificate, many of the Assistant-Mistresses were not. I do not want to say what the minimum of arithmetic should be. For my part I do not see why a girl should not be regarded as cultured even if she cannot get 40 per cent. in the matriculation paper in arithmetic.

Mr. Siddons said that Latin was coming back into its own. I wonder whether that is due to its no longer being the compulsory subject it once was. Our own subject will not suffer, but, on the contrary, its interests will be very much advanced if we do not insist on everybody being examined in it.

The **Chairman**: Before asking Mr. Bolton and Mr. Siddons to reply, I would like to say I believe Mr. Siddons does not want to overstress the fact that examiners may be mistaken. The question is, how to get the nearest uniformity.

Professor **Joliffe:** There is one personal question I would like to ask Mr. Siddons. He referred to something very deplorable which was done by an Oxford don in the War in connection with the schoolmaster who had not taught mathematics. I do not know whether he means me. He was associated with me, and I never set anything alone; he was with me, and so was Professor Godfrey.

Mr. **Siddons:** I protested to the Joint Board about a paper three or four years ago.

Mr. **Joliffe:** If it is that, it does not concern me.

The **Chairman:** I do not think this is very relevant to the discussion. The question is as to how to preserve a fairly uniform standard. I think some of the most important points raised were those by Miss Waters. She suggested, as possibilities, first, that the certificate for university purposes might be of a different standard from the school leaving certificate for going into life. And the other point, which was also mentioned by a gentleman who spoke, was that under certain circumstances it might be wise to allow mathematics to be among the optional subjects for certain purposes. There are three courses. If it is compulsory, may you make a different standard for the university certificate which universities will accept; another for the leaving certificate which commercial firms will accept? And, thirdly, will it be made optional or not? Did I understand Mr. Katz to say that the non-specialist was not likely to have a sufficient income to need to be worried over the income-tax return? (Laughter.)

I call upon Mr. Bolton, and then Mr. Siddons, to reply.

Mr. **Bolton:** I have very little to add. One point was raised by Mr. Taylor in connection with the provision of a practical mathematics course. I am bound to say I am a little uneasy about these practical mathematics courses. I think there is a certain danger of artificiality which, to a large extent, defeats the purpose of those courses. I think it is possible to provide a more effective course of mechanics, in which I should go beyond the limits of the ordinary elementary work that is usually done. I think that with boys of fourteen or fifteen years of age it is possible to devise an effective introduction to practical mathematics through mechanics of a real engineering character. I do not think it is necessary to confine that course to the use of appliances which the boy fully understands. Experiments of that kind can be made with motors, dynamical machines, and so on, so getting a little outside the ordinary limits which are imposed by statics, so commonly the only basis of instruction at that stage. This would afford an admirable introduction to problems which have crept into algebra textbooks concerned with the use and the manipulation of formulae; and it is of importance in relation to making mathematics a subject of more vital interest to boys and girls than it is at present.

Mr. **Siddons:** One or two of the things I said have been rather twisted in the interpretation which has been given to them. Take, for instance, the Oxford and Cambridge Joint Board; I want to say this: that I consider the Oxford and Cambridge Board the best body in examining elementary mathematics at the present day, and any criticisms I make on their work I make because I want to see what is good already made better still. My criticisms are not of the Oxford and Cambridge Joint Board; when I gave examples from examination papers, they were not all from the Oxford and Cambridge Board.

I still think it would be a good thing if boards had a moderator; but apparently they have now got a schoolmaster with practical school experience, and I am glad to hear it.

I was asked who the "right boy" was. I shall not define him. But I know that Jones has no business to pass, and so if Jones gets credit, the wrong boy has got it. Smith, on the other hand, I know is a boy who should

pass, and yet he may come out a long way below the pass mark. When you have a few cases of that sort in a school, you begin to wonder how it is. I mark out the boys whom we expect to pass and which boys may fail, and, in general, the boys we expect to pass do pass, and those we expect to fail, fail. And we get intermediate cases. Sometimes things go wrong, and then we blame the examiner, and, often, the examiner blames us. Often, however, we foresee it.

With regard to the matter of easier questions, I did not mean easier so much as more straightforward. (**The Chairman:** Do you want more routine questions, or questions to make the pupil think?) I believe you want questions which will make the pupil think, but not questions involving heavy technique. Twenty years ago people spoke of "sky-scraper fractions," that is, fractions of more than four storeys. We do not want things of that sort now. I quite agree you must have technique, but there are limits, and it is hard to get some teachers away from technique.

The difficulty in regard to girls' schools has been mentioned, and it exists. In both girls' and boys' schools it is difficult to find those who will give ideas and confine technique to its proper place.

The Chairman: A point was made by Mr. Taylor in regard to the example which was given, namely, that it could be dealt with by simplifying each fraction and then simplifying the result. It is curious how, in that way, a difficult question can be made into an easy one.

I think we may consider that we have had a very successful meeting, but it is a little pathetic in regard to our President.

A vote of thanks to the Chairman was carried by acclamation and briefly acknowledged by him.

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496. We all have to be our own centre, but we are not obliged to be our own circumference.—Dean Inge, *v. Punch*, Aug. 17, 1927.

497. One could write an entirely new book on the wonderful properties of these numbers, but I must resign myself at this place and pass over this with closed eyes.—Stifel, *Arithmetica Integra*, 1544 (seventy years before Napier).

498. In 1807, at the outset of his career, Gauss wrote to Laplace explaining that he was deprived of his fortune and threatened with extreme measures if he did not pay 2000 francs as a war contribution to the French army occupying Göttingen, and representing his colleague Harding as in the same plight. Laplace appealed to Napoleon, but without success. He wrote to Gauss, paid the 2000 francs, and begged his friend to disquiet himself no further. Meanwhile Gauss had been able to borrow the money from Olber and was thus in a position to succour Harding. Two years later he was able to repay Laplace the sum he had borrowed.—*Nature*, July 19, 1877, vol. 16, p. 237.

499. [The name] *Andrew* was something like a negative quantity in Algebra with him;—'twas worse, he said, than nothing.—Sterne's *Life and Opinions of Tristram Shandy*, vol. i. p. 59 (Dent), 1894.

500. [My uncle Toby] proceeded next to *Galileo* and Torricellius, wherein, by certain Geometrical rules, infallibly laid down, he found the precise part to be a PARABOLA—or else an HYPERBOLA,—and that the parameter, or *latus rectum*, of the conic section of the said path, was to the quantity and amplitude in a direct *ratio*, as the whole line to the sine of double the angle of incidence, formed by the breech upon an horizontal plane;—and that the semiparameter,—stop! my dear uncle Toby—stop!—*Loc. cit.* i. p. 90.

501. Most of the young people in Derby or the neighbourhood, if parents could afford it, had lessons in mathematics, penmanship and ciphering from George Spencer (the father of Herbert Spencer)—a necessity and an agreeable one, though he talked more than he taught.

A GEOMETRICAL CONSTRUCTION FOR EPI- AND HYPO-CYCLOIDS.

In the *Gazette* for January, 1925, I gave a geometrical construction for a cardioid. The curve was obtained as the envelope of chords of a circle drawn to a certain rule. It has recently come to my notice that the rule can be extended to cover epi- and hypo-cycloids.

The analysis is as follows :

The pedal equation of an epicycloid is well-known to be :

$$r^2 = a^2 + \frac{4(a+b)b}{(a+2b)^2} \cdot p^2,$$

where a is the radius of the fixed, and b the radius of the moving circle.

In Fig. 1, OR is perpendicular to the chord PQ of the circle, centre the origin, radius c, and angles XOR, POR, measure α and $n\alpha$ respectively.

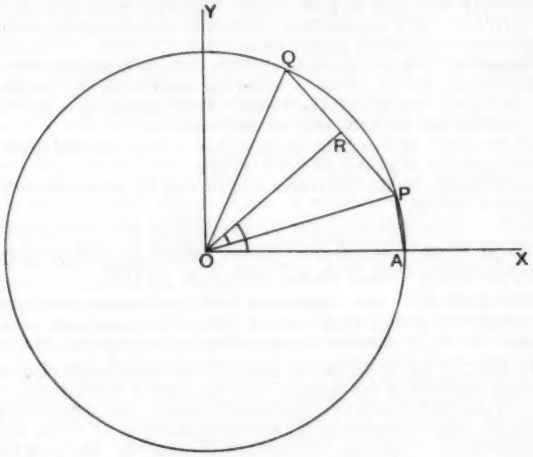

FIG. 1.

The equation of PQ is

$$x \cos \alpha + y \sin \alpha = p = c \cdot \cos n\alpha.$$

To obtain the envelope of PQ, eliminate α between this and

$$-x \sin \alpha + y \cos \alpha = -nc \sin n\alpha.$$

Whence

$$\begin{aligned} r^2 &= p^2 + n^2(c^2 - p^2) \\ &= n^2c^2 + p^2(1 - n^2). \end{aligned}$$

The envelope is, therefore, an epicycloid in which

$$1 - n^2 = \frac{4(a+b)b}{(a+2b)^2},$$

and, therefore,

$$n = \frac{a}{a+2b} \quad \text{and} \quad a = nc.$$

From the figure,

$$Q\hat{O}P : P\hat{O}X = 2n : 1 - n = a : b.$$

Therefore

$$\frac{Q\hat{O}X}{P\hat{O}X} = 1 + \frac{a}{b}.$$

Whence, if $Q\hat{O}P$ be taken equal to $P\hat{O}X$ we get a cardioid, for which $n=\frac{1}{3}$ and therefore, $a=\frac{1}{3}c$.

This was the construction given in the previous note, suggested by the fact that the cardioid is the caustic by reflection from the circle of rays proceeding from a point-source at A.

To extend the construction to the case of an epicycloid of three cusps, join points on the circle whose polar angles are 10°, 40° ; 20°, 80° ; 30°, 120° ; and so on. These chords will envelop the curve. The best method is to divide the circle clockwise into 36 equal arcs, labelling round the points of division from 0 to 35. After a few chords have been drawn the construction becomes a matter of routine and is quickly completed. The result is shown in Fig. 2.

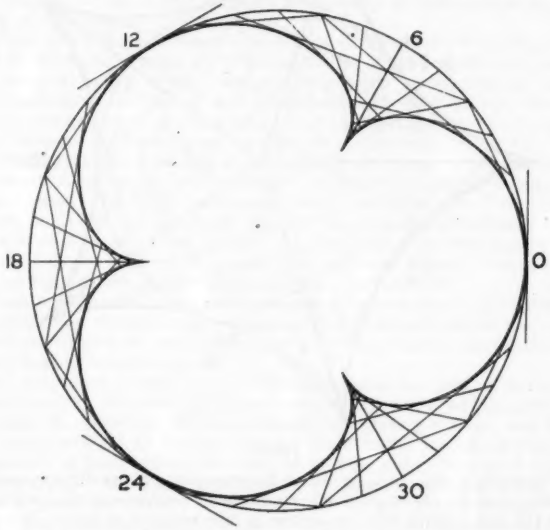

FIG. 2.

For the cardioid, I gave in the previous note an easy construction also for the points of contact of the chords with the envelope. Though many relations can be found between x, y, a, and n, from the first two equations, I have not discovered one that will give a corresponding construction for the general case.

To draw hypocycloids we consider b to be negative.

If we require a five-cusped hypocycloid, we have $\frac{a}{b}=-5$.

$$\therefore\ Q\hat{O}X=-4P\hat{O}X.$$

If, therefore, we graduate the circle also negatively from 0 to -35 and join points 1, -4 ; 2, -8 ; 3, -12 ; ... we get chords enveloping the curve. (Fig. 3).

The credit for the discovery—or rediscovery—of this construction is due to a Dartmouth Cadet to whom I had shown the construction for the cardioid and who then experimented on similar lines. If we do not, in our teaching, despise accurate drawing, many suggestive results, such as this, can be produced by a pupil who has a flair for experimenting and is armed only with ruler,

compasses and protractor. It may be that he is at a stage at which he could not initiate or even follow the corresponding analysis; the experimental result then forms an object to which subsequent teaching in analysis can be directed.

I have called the above a rediscovery, for I have since found a paper read by the late Professor Wolstenholme to the London Mathematical Society in

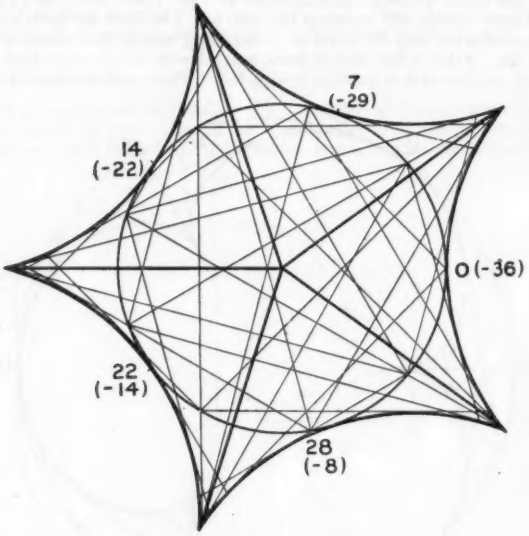

FIG. 3.

1873 (*Proceedings*, vol. iv.), in which he considers epi- and hypo-cycloids as the envelopes of the chord joining instantaneous positions of two points which describe the same circle with velocities in the ratio of m to n. His method of proof is geometrical and very ingenious, though, of course, longer than the above.

ADDENDUM.

Since this note was in type, Mr. R. M. Milne has shown to me the following construction for the point of contact of the chord with its envelope.

Add to Fig. 1 the point X lying between R and P so that $RX = n \,.\, RP$, $RX^2 = n^2(OP^2 - p^2) = n^2(c^2 - p^2)$. But $r^2 = p^2 + n^2(c^2 - p^2)$.

$\therefore\ r^2 = p^2 + RX^2 = OX^2$. So X is the point of contact of the tangent PQ.

Also
$$\frac{PX}{XQ} = \frac{RP - RX}{RP + RX} = \frac{1-n}{1+n}.$$

So for the three-cusped epicycloid, fig. 2, $\dfrac{PX}{XQ} = \dfrac{1}{4}$.

for the five-cusped hypocycloid, fig. 3, $\dfrac{PX}{XQ} = -\dfrac{1}{4}$.

H. E. PIGGOTT.

R.N. College, Dartmouth, *December* 1927.

A CONVERSATION ON RIGHT ANGLES.

By Col. T. C. J. Elliott.

Following a method more popular in the seventeenth century than now, I propose to explain certain views on the definition of a right angle by an imaginary conversation between an inventor of mathematical models and a student.

Student. I suppose you are going to talk about those gratings and lattices made of wire?

Inventor. Yes, and I want you to notice first that, in this equally spaced grating, the two sets of straight wires which cross each other at right angles, like the lines on squared paper, are coloured white and red. Now I am going to define right angle by saying that it is the angle between the two sets of wires.

S. I can see no use in that, as they are already at right angles in Euclid's meaning of right angle.

I. In the physical space known to Euclid all directions are alike, and it is natural to define right angle by the congruence of four angles at a point in a plane, using in the definition the meaning already assigned to "straight line" (practically, the line of a stretched string). And, as you know from Cartesian geometry, we could equally well define straight line by a linear relation between the rectangular coordinates. But when, as in this grating, we can distinguish between the dimensions of the space, it is natural to use that distinction to define "right angle." We define it as the angle between the dimensions. We also make each white and red line "straight" by definition, and then use the linear relation between the coordinates to define the straightness of any other "line" in the space. In this second grating the two sets of wires are no longer at right angles in Euclid's sense, but they are still at right angles in the sense explained above.

S. I can see that the acute and obtuse angles corresponding to the former right angles must be called right angles according to your definition, and that the former straight lines are still straight according to you. But does Euclid's geometry hold in the new space?

I. Not only does it hold, but it still holds when the grating is bent as in this third model. Our straight lines satisfy the linear relations between the coordinates, and therefore we must still call the grating a plane, but the lines are no longer straight in Euclid's sense. That fourth model, in which there are three sets of lines, white, red, and blue, is intended to extend the above definitions of right angle and straight line to three-dimensional space.

S. As you are determined to call the cells of the second grating squares, I suppose you also say that the diagonals are equal to each other, in defiance of the evidence of a footrule?

I. Yes. We treat the footrule as a partially discredited witness, and believe its evidence only when it lies along a red or a white line.

S. But in this fifth model I see that the spacing is quite irregular. Now can Euclid's reasoning be applied here?

I. We can make all the intervals between two consecutive white wires equal by definition, and moreover equal to all the intervals between any two consecutive red wires. Thus to get such a coordinate distance as $x = \text{plus } 7$ from any point is merely a matter of counting seven intervals.

S. But then a distance such as $x = \text{plus } 7\frac{1}{2}$ seems to be meaningless?

I. If we suppose, say, a green wire to be inserted between each pair of red wires then we can make these green wires bisect the interval between the red wires by definition; and, similarly, if we insert two green wires, we can make them trisect the interval. Thus any rational distance can be represented in a model, if we do not mind the complexity. We cannot represent a continuous space, but we can make the gap our minds have to jump as small as we please. We are helped by the fact that the gap is of a kind we are familiar with, for a graduated ruler only permits us to lay off *rational* distances along a Euclidean straight line, so that any model we construct from numerical data

must as a rule differ from what we want to represent much as a dotted line differs from a continuous curve.

S. Admitting you are right, I am still unable to see what purpose is served by this juggling with language. What advantage have your coordinates over ordinary Cartesian coordinates

I. That is like asking what advantage has a thermometer over a clock. Their purposes are quite different. The great value of the Cartesian method lies in its visualising of the path of functions, but the object of these models is to convey certain mathematical ideas, namely those which naturally suggest themselves when the dimensions of a space can be readily distinguished.

S. Is there anything in nature corresponding to your coloured wires ?

I. Certainly. For example, we can call the readings of two physical quantities, such as time or temperature, dimensions, and the totality of pairs of readings a " space " of two dimensions. The dimensions of this " space " are as distinct as the two colours in our second grating, and we are in no danger of confusing a difference in time with one in temperature. We therefore say that the dimensions are at right angles by definition. (An apparatus for illustrating this idea is described in the *Journal of Scientific Instruments*, Oct. 1927.)

S. Your models seem to attenuate progressively the meaning of " space " until there is nothing left.

I. On the contrary, the dimensions are left, and they are all that is necessary. For extension in the physical sense is not an essential part of the idea of " space." This fact enables us to widen the scope of geometry to include space of n dimensions.

S. But, with your use of " right angle," apparently you would call the two sides of any one-to-one function at right angles to each other. For example, you are fond of using an English-French dictionary as an illustration of a one-to-one function, but are you prepared to say that the English language is at right angles to French ?

I. Certainly.

S. Then again, the area of a rectangle has a one-to-two correspondence to the two lengths of two adjacent sides ; now Euclid admits that the two adjacent sides are at right angles to each other, but it seems you would add that the area is at right angles to them both, since we are here dealing with a connection between three variables. I understand you to say that the three variables form a " space," and you call the connection a surface in that space.

I. Yes. But you must remember that it is the series of areas which is at right angles to the two series of sides. There is a chance of misunderstanding if you merely say that the area is at right angles to the sides.

S. Surely you do not expect mathematical teachers to swallow such a tough morsel as that ?

I. They swallowed it at the end of the eighteenth century ; it is only digestion that is wanting. When Lagrange added time to the three dimensions of physical space, this was making time at right angles to them *by definition*. If it was not at right angles by definition I should be glad to know in what sense it was at right angles ? The step to making *all* variables at right angles to each other is obvious, and provides an elementary way of unifying algebra with geometry, for there is then no need to distinguish between such words as line and one-to-one function.

S. Is there not a danger of confusion if we use " right angle " in two distinct meanings ?

I. Perhaps. But mathematics is so full of such snares already that one more can hardly matter. For example, consider in how many ways the word " product " is used.

S. I suggest *wrong angle* as a name to distinguish the meaning we have been talking about.

I. Then you must admit that wrong is sometimes right, as in the first model.

T. C. J. ELLIOTT.

APPROXIMATE EVALUATION OF $\int \text{arc sin } F(t)\,dt$.

By V. Naylor, M.Sc.

There are not many integrals of the type $\int \text{arc sin } F(t)\,dt$ whose values can be expressed in terms of functions already defined. Nor is the inverse sine function allowed to play its free part in suggesting what new functions are necessary for the evaluation of integrals. The moment an inverse sine appears in an integral, the integrand is transformed into one involving the direct function.

Let us define arc sin a as the arc cut off in a circle radius $\frac{1}{2}$, by a chord of length a. This agrees with the usual definition, since (Fig. 1) $A\hat{O}B$ is twice the angle whose sine is a and arc $AB = \frac{1}{2} \times A\hat{O}B$.

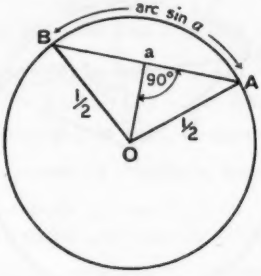

Fig. 1.

The addition formula for the inverse sine function can be readily deduced. In a circle, centre O, radius $\frac{1}{2}$ (Fig. 2), let chord $AB = a$, chord $BC = b$, chord $AC = c$.

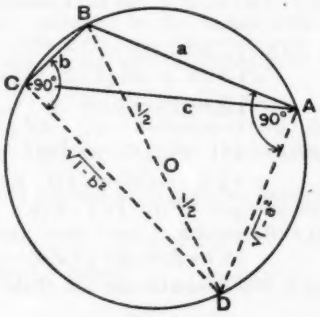

Fig. 2.

Complete the figure as shown.

$$AD = \sqrt{1-a^2}, \quad CD = \sqrt{1-b^2},$$

$$\text{arc sin } a + \text{arc sin } b = \text{arc } AB + \text{arc } BC$$
$$= \text{arc } AC$$
$$= \text{arc sin } c.$$

We now require c in terms of a and b.
By Ptolemy's theorem :

$$b\sqrt{1-a^2}+a\sqrt{1-b^2}=c\times 1.$$

Hence $\quad \text{arc sin } a+\text{arc sin } b=\text{arc sin }(b\sqrt{1-a^2}+a\sqrt{1-b^2}).$

We now obtain an approximation to the inverse sine. In a circle, centre O radius $\frac{1}{2}$ (Fig. 3), let

$$\text{chord } AB=F(t).$$

Then $\quad \text{arc } AB=\text{arc sin } F(t),$

$$\hat{AOB}=2 \text{ arc sin } F(t)=\theta, \text{ say.}$$

Thus $\quad \int \text{arc sin } F(t)\,dt=\int \text{arc } AB\,dt.$

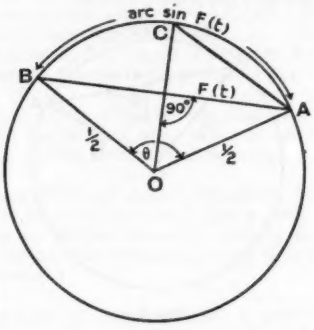

FIG. 3.

We employ Huyghen's approximation to the arc in terms of θ,* which, by means of the relation $\theta=2 \text{ arc sin } F(t)$, can be expressed back in terms of t.
With the usual figure we have

$$\begin{aligned}\text{arc } AB &\sim \tfrac{1}{3}(8AC-AB)\\ &=\tfrac{1}{3}(8\,.\,2\,.\,\tfrac{1}{2}\,.\,\sin\tfrac{1}{2}\theta-2\,.\,\tfrac{1}{2}\,.\,\sin\tfrac{1}{2}\theta)\\ &=\tfrac{1}{3}(8\sin\tfrac{1}{2}\theta-\sin\tfrac{1}{2}\theta).\end{aligned}$$

But $\quad 2\sin^2\tfrac{1}{4}\theta=1-\cos\tfrac{1}{2}\theta=1-\sqrt{(1-\sin^2\tfrac{1}{2}\theta)}\,;$

$$\begin{aligned}\therefore\ \sqrt{2}\,.\,\sin\tfrac{1}{4}\theta &=\sqrt{[1-2\sqrt{\{\tfrac{1}{4}(1-\sin\tfrac{1}{2}\theta)(1+\sin\tfrac{1}{2}\theta)\}}]}\\ &=\sqrt{\tfrac{1}{2}(1+\sin\tfrac{1}{2}\theta)}-\sqrt{\tfrac{1}{2}(1-\sin\tfrac{1}{2}\theta)}.\end{aligned}$$

Thus $\quad \sin\tfrac{1}{4}\theta=\tfrac{1}{2}\sqrt{1+F(t)}-\tfrac{1}{2}\sqrt{1-F(t)}.$

Hence $\quad \text{arc sin } F(t)=\text{arc } AB$

$$\sim \tfrac{1}{3}\sqrt{1+F(t)}-\tfrac{1}{3}\sqrt{1-F(t)}-\tfrac{1}{3}F(t).$$

The errors involved in this approximation are of the same order as those in Huyghen's Rule.
E.g. relative error

$$< 1 \text{ in } \quad 6{,}000 \text{ if } \theta=\tfrac{1}{3}\pi, \text{ i.e. } \tfrac{1}{3}\pi=\text{arc sin } F(t),\ F(t)=\tfrac{1}{2}.$$

$$< 1 \text{ in } 20{,}000 \text{ if } \theta=\tfrac{1}{4}\pi, \text{ i.e. } \tfrac{1}{4}\pi=\text{arc sin } F(t),\ F(t)=\sin\tfrac{1}{4}\pi.$$

$$< 1 \text{ in } 100{,}000 \text{ if } \theta=\tfrac{1}{6}\pi, \text{ i.e. } \tfrac{1}{12}\pi=\text{arc sin } F(t),\ F(t)=\sin\tfrac{1}{12}\pi.$$

* Gibson, *Introduction to the Calculus*, p. 116 ; Caunt, *Infinitesimal Calculus*, p. 478.

Let us apply the method to a function whose integral can be found; *e.g.*

$$\int \text{arc sin}\sqrt{x}\,dx.$$

For the approximate value, we have

$$\begin{aligned}\int \text{arc sin}\sqrt{x}\,dx &\sim \int \{\tfrac{4}{3}\sqrt{1+\sqrt{x}} - \tfrac{4}{3}\sqrt{1-\sqrt{x}} - \tfrac{1}{3}\sqrt{x}\}\,dx\\ &= \int \tfrac{4}{3}\,.\,2y\,.\,\sqrt{1+y}\,dy - \int \tfrac{4}{3}\,.\,2y\,.\,\sqrt{1-y}\,dy - \int \tfrac{1}{3}\sqrt{x}\,dx\\ &= \tfrac{8}{3}[\tfrac{2}{3}y(1+y)^{\frac{3}{2}} - \tfrac{2}{3}\,.\,\tfrac{2}{5}(1+y)^{\frac{5}{2}}]\\ &\quad - \tfrac{8}{3}[-\tfrac{2}{3}y(1-y)^{\frac{3}{2}} - \tfrac{2}{3}\,.\,\tfrac{2}{5}(1-y)^{\frac{5}{2}}] - \tfrac{2}{9}x^{\frac{3}{2}}\\ &= \tfrac{1}{45}[\sqrt{x}(1+\sqrt{x})^{\frac{3}{2}} - \tfrac{2}{5}(1+\sqrt{x})^{\frac{5}{2}} + \sqrt{x}(1-\sqrt{x})^{\frac{3}{2}}\\ &\quad + \tfrac{2}{5}(1-\sqrt{x})^{\frac{5}{2}}] - \tfrac{2}{9}x^{\frac{3}{2}}.\end{aligned}$$

For the relative error to be less than 1 in 6000 we must have $F(t) \leqslant \frac{1}{2}$; *i.e.* $x \leqslant \frac{1}{4}$.

Taking limits of integration to be 0 and $\frac{1}{4}$, the approximate value for $\int_0^{\frac{1}{4}} \text{arc sin}\sqrt{x}\,dx$ becomes $-\frac{2}{135}\sqrt{6} + \frac{14}{48}\sqrt{2} - \frac{1}{360} = 0{\cdot}0856012$.

For the true value, integrating by parts, we have

$$\begin{aligned}\int \text{arc sin}\sqrt{x}\,dx &= x\,\text{arc sin}\sqrt{x} - \int \sin^2 u\,du,\quad u = \text{arc sin}\sqrt{x}\\ &= x\,\text{arc sin}\sqrt{x} - \tfrac{1}{2}u + \tfrac{1}{4}\sin 2u\\ &= x\,\text{arc sin}\sqrt{x} - \tfrac{1}{2}\,\text{arc sin}\sqrt{x} + \tfrac{1}{2}\sqrt{x}\sqrt{1-x}.\end{aligned}$$

Hence $$\int_0^{\frac{1}{4}} \text{arc sin}\sqrt{x}\,dx = \tfrac{1}{8}\sqrt{3} - \tfrac{1}{24}\pi = 0{\cdot}08560665.$$

A large number of integrals, which can be made to depend on

$$\int \text{arc sin}\,F(t)\,dt,$$

can be approximately evaluated by this method. Thus, if we use the fact that integration by parts gives us a connection between the integrals of a function and its inverse, we can use this method to integrate powers of $\sin\theta$, subject to certain conditions.

E.g. writing $y = \sin^{\frac{1}{2}}\theta$, $\theta = \text{arc sin}\,y^2$, we have

$$\int \sin^{\frac{1}{2}}\theta\,d\theta = \theta\sin^{\frac{1}{2}}\theta - \int \text{arc sin}\,y^2\,dy.$$

If $y^2 \leqslant \frac{1}{2}$, *i.e.* $y \leqslant \frac{1}{2}\sqrt{2}$, and so, $\theta \leqslant \frac{1}{4}\pi$, we have an error less than 1 in 6000 in the result.

Integrating between values 0 and $\frac{1}{8}\pi$ for θ,

$$\begin{aligned}\int_0^{\frac{1}{8}\pi} \sin^{\frac{1}{2}}\theta\,d\theta &= \Big[\theta\sin^{\frac{1}{2}}\theta\Big]_0^{\frac{1}{8}\pi} - \int_0^{\frac{1}{4}\sqrt{2}} \text{arc sin}\,y^2\,dy\\ &\sim \tfrac{1}{192}\pi\sqrt{2} - \int_0^{\frac{1}{4}\sqrt{2}} \{\tfrac{4}{3}\sqrt{1+y^2} - \tfrac{4}{3}\sqrt{1-y^2} - \tfrac{1}{3}y^2\}\,dy\\ &= \tfrac{1}{192}\pi\sqrt{2} - \tfrac{4}{3}\Big[\tfrac{1}{2}y\sqrt{1+y^2} + \tfrac{1}{2}\,\text{arc sinh}\,y - \tfrac{1}{2}y\sqrt{1-y^2}\\ &\qquad - \tfrac{1}{2}\,\text{arc sin}\,y - \tfrac{1}{12}y^3\Big]_0^{\frac{1}{4}\sqrt{2}}\\ &= 0{\cdot}37 - 0{\cdot}1196 = 0{\cdot}2507.\end{aligned}$$

With limits of 0 and $\frac{1}{2}\pi$, we have the Γ-function available for integrating powers of $\sin\theta$, but with other limits we are forced to find the indefinite integral, failing which we must resort to approximate integration.

If necessary, the range of integration may be divided up into such portions as will ensure that the arcs can be represented by Huyghen's Rule within the prescribed degree of accuracy.

Thus $$\int_0^{\frac{1}{2}\pi} \sin^{\frac{1}{2}}\theta\, d\theta = \left\{\int_0^{\frac{1}{6}\pi} + \int_{\frac{1}{6}\pi}^{\frac{1}{3}\pi} + \int_{\frac{1}{3}\pi}^{\frac{1}{2}\pi}\right\} \sin^{\frac{1}{2}}\theta\, d\theta$$

would lead to $$\left\{\int_0^{\frac{1}{4}\sqrt{2}} + \int_{\frac{1}{4}\sqrt{2}}^{\frac{1}{4}\sqrt{3}} + \int_{\frac{1}{4}\sqrt{3}}^{1}\right\} \text{arc sin } y^2\, dy,$$

and we should then transform the last two integrals so as to bring the variable within the range 0 to $\frac{1}{2}\sqrt{2}$.

One final example. Integrating by parts,

$$\int \log\sin\theta\, d\theta = \theta\log\sin\theta - \int \theta\cot\theta\, d\theta.$$

Also, writing $y = \log\sin\theta$, $\theta = \text{arc sin } e^y$,

$$\int \log\sin\theta\, d\theta = \theta\log\sin\theta - \int \text{arc sin } e^y\, dy.$$

Hence $\int \log\sin\theta\, d\theta$ and $\int \theta\cot\theta\, d\theta$ for certain ranges of integration can be made to depend on

$$\int \text{arc sin } e^y\, dy \sim \int \{\tfrac{4}{3}\sqrt{1+e^y} - \tfrac{4}{3}\sqrt{1-e^y} - \tfrac{1}{3}e^y\}\, dy,$$

which, by writing $1+e^y = x^2$ and using partial fractions, can be shown to be

$$\tfrac{4}{3}\left[2\sqrt{1+e^y} + \log\frac{\sqrt{1+e^y}-1}{\sqrt{1+e^y}+1}\right] - \tfrac{4}{3}\left[2\sqrt{1-e^y} + \log\frac{\sqrt{1-e^y}-1}{\sqrt{1-e^y}+1}\right] - \tfrac{1}{3}e^y.$$

H.M. Dockyard School, Devonport. V. Naylor, M.Sc.

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502. The only Northern state likely at present to ban evolution is Indiana. . . . It was the Indiana Legislature which, in 1899, very nearly passed a Bill that in all Indiana schools π should be 4 instead of 3·14159. It was in Goshen, also in Indiana, that a gardener who last year hybridised two species of gladiolus was expelled from his Church for interfering with God's species.—(*Times Literary Supplement*, Sept. 1st, 1927, review of *The War on Modern Science*, by Maynard Shipley, President of the Science League of America.) [Per Mr. C. W. Adams.]

503. I have known a first-rate philosopher and mathematician who mastered the whole science of music . . . yet to whom musical sounds were unintelligible and odious. It was he who once declared that a fine melody "suggested to him the idea of chloride of lime."—Fitzgerald, *The Book Fancier*, p. 92. [Did not Ruskin say that music by Beethoven reminded him of bags of nails dropped on the floor, with occasional blows from a heavy hammer?]

504. Erasmus.—March 1516, Basel.—We have here Henry Glarean . . . extremely skilful in every branch of Mathematics. [He had a gold watch, from E., Ἐράσμος Γλαρεάνῳ (probably the Nuremberg egg), just out.]

Feb. 1517.—Erasmus to Stephen Poucher, Bishop of Paris, from Antwerp.—In mathematics Glarean is most expert: for it is in that sphere that he chiefly reigns.

Beatus Rhenanus writes to Erasmus, saying that Glarean is to have, while in France, 150 frs. a year.

BRITISH ASSOCIATION AT LEEDS, 1927.

Mathematical Department of Section A.

An interesting programme of lectures on a wide range of subjects was given by the Mathematical Department of Section A of the British Association at the Leeds meeting in 1927.

The first paper was contributed by Prof. H. W. Turnbull on Thursday, September 1, and was entitled "Non-commutative Algebra." Prof. Turnbull explained the relative importance of the fundamental laws of algebra, and showed that algebras can exist in which the commutative law expressed by the relation $a \times b = b \times a$ does not hold, the most familiar instance of such an algebra being the algebra of matrices. Prof. Turnbull discussed various types of "non-commutative algebra," and indicated the bearing of these algebras on geometry and quantum physics.

The morning of Friday, September 2, was devoted to synthetic geometry. In a paper on "Noether's Canonical Curves" Prof. W. P. Milne outlined the proof of Noether's theorem that the general plane curve of genus p can be represented point by point on a curve of degree $2p-2$ in space of $p-1$ dimensions, and described the fundamental properties of the "canonical curves" for $p=3, 4, 5, 6$. Prof. Milne's remarks were supplemented by Mr. F. P. White, who indicated a number of interesting questions suggested by Prof. Milne's paper.

On Monday, September 5, Prof. S. Brodetsky spoke on "The Equations of the Gravitational Field in Two and in Three Dimensions of Space-time." Prof. Brodetsky explained how the nature of a gravitational field may be disguised by the coordinates in which the equations are expressed, and discussed (in the case of a field in fewer than four dimensions of space-time) the precautions which must be taken in order to avoid obtaining what is in reality merely a Galilean field.

The morning of Tuesday, September 6, was devoted to papers on various parts of the theory of numbers by Prof. W. E. H. Berwick, Mr. B. M. Wilson, and Mr. A. E. Ingham.

Prof. Berwick, in a paper entitled "The Arithmetic of Cubic Number-fields," gave a general account of the arithmetic of complex integers in a field of algebraic numbers. In such a field the fundamental property of uniqueness of factorisation into primes does not in general hold, as Prof Berwick showed by examples. For this reason certain aggregates of algebraic integers, called ideals, are introduced, and for these it can be shown that (with suitable definitions of multiplication, divisibility, etc.) the property does hold. The general theory was illustrated by numerical examples of the factorisation of ideals in a cubic field, and the important problem of the determination of the "units" of a cubic field was discussed.

This paper was followed by one on "Ramanujan's Work on Congruence Properties of the Number of Partitions of n," by Mr. B. M. Wilson. In studying a table of the values of $p(n)$,* Ramanujan noticed a number of curious congruence relations and was led to conjecture the truth of a certain general theorem. Typical special cases of the theorem are the congruences expressed by the formulae:

$$\left.\begin{array}{l} p\ (5m+4) \equiv 0 \pmod 5 \\ p\ (7m+5) \equiv 0 \pmod 7 \\ p\ (11m+6) \equiv 0 \pmod{11} \end{array}\right\} \quad (m=0, 1, 2, \ldots).$$

Ramanujan never proved his theorem in its general form, but before his death in 1920 had obtained proofs of a number of particular cases, including

* $p(n)$, the number of (unrestricted) partitions of n, is the number of distinct ways in which n can be expressed as a sum of positive integers. Thus $p(4)=5$, since 4 can be expressed in the *five* ways:

$$4=3+1=2+2=2+1+1=1+1+1+1.$$

the three results just quoted. Mr. Wilson, joint-editor with Prof. Hardy and Mr. Seshu Aiyar of Ramanujan's papers, reported on Ramanujan's researches published and unpublished, on this subject, and outlined the methods used by him.

In the concluding paper, entitled "The Analytical Method in the Theory of Numbers," Mr. A. E. Ingham gave a general account of the analytical method of Hardy and Littlewood as applied to Waring's problem. By a known theorem of Lagrange every positive integer can be expressed as a sum of four squares (positive or zero), and Waring had conjectured (in 1770) that there are similar theorems for cubes, fourth powers, and generally for kth powers (the number of powers required depending of course on k). The truth of this conjecture was first established by Hilbert in 1909, and an entirely new proof was later given by Hardy and Littlewood. Hardy and Littlewood, interpreting "Waring's problem" in its widest sense, aim at obtaining definite information about the actual number of cubes, fourth powers, etc., which suffice to express all positive integers (or all large positive integers), and this they are able to do by their very powerful analytical method. Mr. Ingham summarised the results so far obtained towards the solution of Waring's problem, and indicated in general terms the nature of the analytical method of Hardy and Littlewood.

The papers were followed by interesting discussions in which many distinguished mathematicians took part.

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505. He [D. F. Gregory] had long been an admirer of Fourier's great work on heat . . . and once, while turning over its pages, remarked to the writer [Robert Leslie Ellis], "All these things seem to me to be a kind of mathematical paradise."—*Mathematical Writings* of D. F. Gregory, 1865, p. xxi.

506. A Student's "Solution":

$$\frac{5}{x-6}+\frac{4}{x-9}=\frac{8}{x-7}+\frac{1}{x-10}; \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

$$\therefore \frac{x-6}{5}+\frac{x-9}{4}=\frac{x-7}{8}+\frac{x-10}{1}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

And on working out, $x=11$.—*Mathematics Teacher*, Dec. 1925, p. 473.

507. To the statement that mathematics must play an increasing part in the farmer's life, a "Star" sub-editor considered the following headline appropriate: Calculus rack him !—[v. Browning's *Grammarian's Funeral.*]

508. Parts are so fitted unto Parts, as do shew thou hadst wit and mathematics too.—Cartwright, *Upon the . . . Poems of Master John Fletcher.*

509. Henry Bullock, Fellow of Queens' (Fisher, Master), sent a horse to Erasmus to help him in the journey from London to Cambridge. Bullock was the mathematical reader to the University. Erasmus telling Colet of his misfortunes on the journey: Bullock consulted ye stars, and says Jupiter is in an angry mood.

510. At [Benj. Franklin's] second chess party with the agreeable Mrs. Howe [wife of Admiral Lord Howe], after playing as long as they liked, they fell into a little chat, partly on a mathematical problem, for Franklin says, "the lady, which is a little unusual in ladies, had a good deal of mathematical knowledge."—Barrow's *Life of Howe.*

511. Artemas Martin began the *Mathematical Visitor* in 1877. In the number for Jan. 1880 he writes: "This number of the *Visitor* has been delayed some months, in consequence of the sickness of the editor, who has done all the type-setting with his own hands. He is not a practical printer, and never had set up a stickful of type till last May or June."

REVIEWS.

Mathematics for Students of Technology: A Senior Course. By L. B. BENNY. Pp. xxviii + 451. 10s. 6d.: or in parts, Part I. 5s.; Part II. 6s. 1927. (Humphrey Milford, Oxford Univ. Press.)

Those who remember Mr. Benny as one of the critics of the teaching of "Practical Mathematics" whose attacks resulted in the Mathematical Association appointing a sub-committee to explore the situation, will be interested in this book. Mr. Benny favours the acquisition of a sound mathematical equipment first and its application later, and a cursory inspection of the book might leave the impression that there is little of the definitely technical in it. Closer examination will show, however, that in the worked examples, and in the numerous exercises, few of the important applications of mathematics within its scope have been overlooked. Only a few leading examples on each topic are given, but Mr. Benny would probably urge that the *detailed* application is the technical teacher's affair, and not his. It is, however, arguable that the evening students for whom this book is specially written should be convinced that mathematics is worth while, and that their interest should be awakened by being shown the value of the mathematics they are to learn *before* they set to work to learn them.

As Mr. Benny's main objection to "Practical Mathematics" was its tendency to become slipshod, it is natural to turn early to the more "delicate" topics to see how he deals with them. It is a pleasure to admit that he comes through the test with flying colours. His introduction to functionality, and his treatments of fractional and negative indices, limits, and rates, are well contrived and studiously careful.

It is evident throughout that the order of introduction to the various subjects is one of design and not of chance. Refreshing novelties of treatment are met with, such as the encouragement, by example, to discover the salient features of the shape of a graph before commencing detailed plotting; A.P. arising out of graphs of linear laws, and G.P. from $y = kx^n$; and polar coordinates and $r - \theta$ graphs in the chapter on vectors. In the trigonometry sections, however, the order chosen gives us pause. After commencing with radian measure, sine and cosine are introduced as the projections of a rotating line, and we proceed quite naturally to angles of any magnitude, but the use of the ratios in the solution of surveying and geometrical problems appears rather as a happy accident; and at the end of this (first) chapter, the student is asked to solve equations like $x \sin^2 x = 2x - 5$ in radians, graphically. This seems rather a rapid transition. A chapter on vectors follows, containing many good things, and later there are comprehensive sections on practical applications—solution of triangles and mensuration connected with the circle. The sum and difference formulae come late in Part II, although electrical students need them earlier.

In Part I there are good chapters on areas under graphs, on determination of laws, and on solid geometry. Part II, mainly devoted to calculus up to the exponential and logarithmic functions, is all good. The numerous exercises throughout are carefully selected and varied. Diagrams are clear and well arranged, although the straight line on p. 64 does not "lie as evenly as possible among the points plotted," and the "rough graphs" on p. 81 are perhaps unnecessarily rough.

We advise all teachers of technical students to get a copy of this book, and to see how far they can introduce Mr. Benny's excellent mathematical spirit into their work.

W. G. BICKLEY.

Mathematik und Logik. By H. BEHMANN. Pp. 59. 1·20 m. 1927. (Teubner, Leipzig.)

The critical examination of the foundations of analysis and geometry has had much influence on and been influenced by the development of modern logic. This logic, unlike the Aristotelean, is always cast in a completely symbolic form, but unfortunately no system of symbolism has met with general acceptance. Omitting systems—such as Frege's—that no one uses,

we have Peano's in his *Formulario*, its modification in Whitehead and Russell's *Principia*, and that used by Hilbert in his work on metamathematics. All these seem well-designed for the purposes they were invented to serve, Peano's for setting out ordinary mathematics symbolically, Whitehead and Russell's for a deeper analysis of the logical basis of mathematics, and Hilbert's for the investigation of what can be deduced from a given set of axioms. It is a matter of regret that our author has not used any of these schemes, but one of his own instead.

The book deals successively with what an English reader would call elementary propositions, propositional functions, classes, relations, and cardinal arithmetic. The treatment is based on the *Principia*, but the stress is different. No emphasis, *e.g.* is laid on descriptions. There are some interesting remarks, old and new, on the question : given a logical formula, to discover a simple test for its truth or falsehood. The solution is given for the case where only elementary propositions are involved. Except for the unusual nature of the notation, the book is a good introduction to the subject, much more technical than books of this size and price are in this country. Is there no demand here for such "non-popular" but elementary expositions ?

H. G. F.

Gewöhnliche Differentialgleichungen. 2nd edn. By J. HORN. 9 m. 1927. (Walter de Gruyter & Co., Berlin und Leipzig.)

First published in 1905 as volume L of the *Sammlung Schubert*, Horn's introduction to the theory of Ordinary Differential Equations now reappears as the tenth volume of the section of *Göschens Lehrbücherei* devoted to pure mathematics. Presumably because the higher branches of the subject, and in particular those branches which are grafted on to the theory of functions of a complex variable, are adequately dealt with in other German treatises, as for example in Schlesinger's *Einführung*, the author has now suppressed a number of sections which appeared in the original edition. Thus the asymptotic representation of solutions of a linear equation and Painlevé's investigations into equations of the second order whose general solution is free from movable critical points find no place in the present issue. In short, Horn has retained little more than the minimum canon of theory with which every professed mathematician must be acquainted.

The earlier chapters are arranged in the order which writers and teachers have found to be the most satisfactory. The elementary processes by which certain well-defined types of equation can be integrated or at least reduced to integrable form occupy the first chapter, and prepare the reader for the theoretical investigations which follow. In the second chapter the existence theorems are proved, by the method of successive approximations, for the general explicit equation of the first order and for a system of two equations. The discussion of singular solutions of the implicit equation of the first order is brief, but clear. In particular, the fact that such an equation has in general no singular solution is emphasised, a happy contrast to so many elementary text-books, which give the reader the impression that the contrary is true.

The third chapter is devoted to methods of approximation to solutions. On the numerical side, the formulae of Kutta and Runge are given ; they are sound practical methods, which should find a place in every general treatise on Differential Equations. One or two graphical methods are indicated, but they are of interest rather as ingenious devices than as processes of any practical value.

The fourth and fifth chapters introduce the reader to the special theory of linear equations. After making clear the conditions for a fundamental set of solutions, Horn proceeds to discuss the homogeneous linear equation with constant coefficients. No doubt space is saved by the initial assumption of a particular solution of exponential form, but educationally there is much to be said against this procedure. The method of factorising the operator is more logical and more illuminating than the method adopted. The treatment of the general non-homogeneous equation is based upon the method of Variation of Parameters. An account of the corresponding theorems for systems of linear equations of the first order concludes the fourth chapter. A glimpse of

two special aspects of the theory of linear equations is given in the fifth chapter, namely the Sturmian theory of the zeros of solutions of an equation of the second order and the Floquet theory of equations with periodic coefficients, a glimpse just sufficient to make the reader wish for more.

Up to this point Horn has confined himself to equations with real variables: now he takes up the theory anew from the point of view of the complex variable. In the sixth chapter, the corresponding existence theorems are proved, and the mode of dependence of the solution on the one hand upon any parameter which may enter into the equation, and on the other upon initial values, is made clear. The seventh chapter is exclusively devoted to the linear equations in the complex domain. A very complete discussion of the development of solutions in the neighbourhood of a regular singular point according to the classical method of Frobenius is followed by a general treatment of equations of Fuchsian type, which are characterised by the absence of essential singularities, and a particular treatment of the equations of Gauss and Legendre. The Bessel equation serves as an introduction to the class of equations which follows next in order of complexity, namely linear equations whose coefficients admit of expansions in Laurent series in the ring

$$0 < R' < |x| < R.$$

The reader is referred to the other works for a discussion of the asymptotic development of the solutions.

In the eighth chapter Horn returns to equations and systems of equations of the first order in the real domain, and enters upon a complete discussion of a solution regarded as a function of the parameters or of the initial values. This is the only chapter which contains matter of a somewhat specialised nature: it touches upon Horn's own work. An instructive example is given in the equation of one-dimensional small vibrations:

$$\frac{d^2x}{dt^2} + x = a_2x^2 + a_3x^3 + \dots,$$

subject to the initial conditions $t = 0$, $x = c$, $\dot{x} = 0$, where the solution is developed in ascending powers of c and the period is found to be:

$$T = 2\pi\left[1 + (\tfrac{5}{12}a_2^2 + \tfrac{3}{8}a_3)c^2 + \dots\right].$$

The chapter concludes with Poincaré's investigation of conditions for the existence of periodic solutions of systems of equations involving a parameter. Assuming that a solution of period T exists when $\mu = 0$, can the system possess the same period T for small values of μ? The question is of importance in the problem of three bodies.

The ninth and last chapter is devoted to the equation

$$\frac{dx}{X(x, y)} = \frac{dy}{Y(x, y)},$$

where X and Y vanish for $x = y = 0$. The existence and nature of solutions in the neighbourhood of the origin is discussed in considerable detail. The concluding section of the book gives examples of divergent series which formally satisfy differential equations.

The typography is excellent: the page is sufficiently large for the adequate display of the text, yet not too large for the reader's comfort. There are no examples for the reader to solve, but in all other respects the book is an admirable introduction to a branch of Analysis to which the German schools have made, and are still making, outstanding contributions.

E. L. Ince.

Mathematical Statistics. By H. L. Rietz. Pp. xii+181. 10s. 1927. (Open Court.)

This little book by the Professor of Mathematics in the University of Iowa is the third of the Carus Mathematical Monographs. Its main purpose, according to the author, is to make "a slight contribution towards shifting the emphasis and point of view in the study of statistics in the direction of the consideration of the underlying theory involved in certain highly important

methods of statistical analysis," and to introduce "some of the recent advances in mathematical statistics to a wider range of readers." This purpose it admirably fulfils. It is also claimed that considerable portions of the monograph can be read by those who have relatively little knowledge of college mathematics. That perhaps is rather more doubtful. So much ground is covered in the seven short chapters that the treatment is necessarily compressed and often slight, and it would almost certainly prove inadequate for the average student without any previous acquaintance with the subject.

In the first chapter an explanation is given of the kind of problem to be discussed and some simple concepts are defined. In the second, the theorem of Bernoulli is introduced and it is pointed out that, with an appropriate definition of probability as a limit, it requires no proof. Then follows an account of the fundamental theorem of Laplace, with which De Moivre's name is linked as a result of Karl Pearson's historical researches, and of Poisson's exponential function under the not inappropriate title of the "Law of small probabilities."

Chapter III. is devoted to a discussion of generalized frequency curves with special attention to two systems. The Pearsonian system is first developed along the usual lines, which are well enough known to need no mention. The genesis of the Gram-Charlier series is afterwards described, but the discussion of the underlying probability theory is relegated to the last chapter. These curves are derived by utilising the normal frequency function and the Poisson exponential function, respectively, as rough approximations to a given frequency distribution, and then building up a rapidly converging series of which the first term is the selected frequency function.

In the next chapter the subject of correlation is treated by the method of regression and the correlation surface method, starting with two variates and leading on to multiple and partial correlation. This account does not pretend to be complete. For instance, the reader is referred elsewhere for information as to how to deal with characters which admit of classification but not of exact measurement. That kind of case is one which frequently arises, especially in the application of statistical analysis to social problems.

A chapter on random sampling follows. A few of the better known standard error formulas are established in order to bring out the main assumptions and approximations involved. In the application and interpretation of the probable error a warning is given as to the size of samples, and stress is rightly laid on the restriction that the distribution of the statistical constant under review must be normal. Some progress has been made towards the removal of this restriction by a different method of approach. The author indicates the line of research followed by Pearson and Camp, each starting with a generalized form of the Bienaymé-Tchebycheff criterion, better known under the name of the Tchebycheff inequality. A brief account of the Lexis theory is given in Chapter VI.

The author is to be congratulated on his success in presenting so clear and interesting an exposition of his subject within so small a compass. A useful index and a list of references are provided, and the print and binding of the book are very neat. With a view to the next edition slight misprints or omissions may be noted on p. 37, l. 15; p. 136, last equation; p. 166, l. 3.

D. CARADOG JONES.

Newton's Interpolation Formulas. By DUNCAN C. FRASER. Pp. iv + 95. 8s. 6d. 1927. (Layton.)

Mathematicians and especially actuaries owe a debt of gratitude to the author for putting together in a convenient form all that Newton is known to have written on the application of finite difference formulas to interpolation questions. It is unnecessary to describe Newton's methods as they are already familiar to those who have studied Mr. Fraser's article in the Newton Memorial Volume so recently published, but it may be useful to summarise the contents of the present work. It comprises five parts as follows: The *Methodus Differentialis*, a small tract embracing six propositions and a scholium on central formulas of interpolation and their application. A letter describing a method for computing tables of square roots, cube roots and

fourth roots of numbers. The 5th Lemma in Book III. of the *Principia*: *To find a parabolic curve which shall pass through any given points.* References to the subject of interpolation and finite differences in a collection of letters bearing on the controversy between Newton and Leibniz as to the origin of the differential calculus. A photographic reproduction, together with a transcription and translation of a manuscript, *Regula Differentiarum*, discovered by the author in the University Library at Cambridge, which, as he justifiably claims, adds materially to our knowledge of the work done by Newton on the subject of interpolation. In addition there are a number of illuminating notes, historical and otherwise. D. CARADOG JONES.

The Mathematics of Engineering. By R. E. ROOT. Pp. xiii + 540. 34s. 1927. (Ballière, Tindall and Cox.)

It is not easy to take a detached or dispassionate view of a book, which is accompanied by a printed leaflet from the publishers, that informs us that, "*it is a book which Engineering Schools have needed for many years, and which has never before been so admirably supplied.*" In addition, "*it covers the whole range of Mathematics essential to the Engineer, whatever may be his speciality. If you are a teacher you will find it a book consciously planned to fill a long felt need of Engineering education and one eminently teachable. For more than thirteen years the author has continued to sift, weigh and add to the growing body of Mathematics, as class experience dictated.*" What more need be said? If good wine needs no bush, what can be said of vintage that is accompanied with a whole thicket of ivy?

The book seems to the reviewer to be a compendium of Mathematics with occasional references to practical applications: the greater part does not differ materially from what is to be found in standard text-books. Such a work as Perry's *Calculus for Engineers* contains many more strictly Engineering problems than are to be found here. The second Chapter on Limits and Continuity, important as the subject is from a theoretical standpoint, seems somewhat out of place in a work of this kind. The chapters on Complex Variables, Periodic Functions and the Theory of Probability will be found useful by those who have no access to books on these topics. The five chapters on Differential Equations which come at the end are the most complete, and contain much work that is only to be found in special treatises.

The author has compressed into some 500 pages a wide range of subjects: many of his proofs are so condensed that a student reading the work by himself might find some difficulty in following the argument. With the assistance of a guide, philosopher and friend in the shape of an efficient teacher his difficulties would probably disappear. It is remarkable that a work, which apparently has been used in manuscript form for some years by Dr. Root's pupils, should not contain answers to the examples. To the unaided student the value of the book would be much enhanced if the answers were given. R. M. M.

The Internal Constitution of the Stars. By A. S. EDDINGTON, M.A., F.R.S. Pp. viii + 407. 25s. 1926. (Cambridge University Press.)

For an overwhelmingly large majority of mankind stars shine "because they are stars." Even for the astronomers of old, using them as trigonometrical points for the survey of the solar system, it was enough that the lamps seemed to be kept constantly burning. The study of the proper motions of the "fixed" stars by statistical methods disclosed to modern astronomers the motion of the solar system as a whole, and lifted the veil that hid the immensity of the stellar universe. Still more recently the astrophysicists have collected data about the various features in the spectra of individual stars and about the motions of the stars—data in sufficient quantity and precision to allow of the application of statistical generalisations, till at last, by the astounding rapidity with which physicists have extended our knowledge of the processes involved in atomic structure, it has become possible to bind the study of the minutely little with the investigation of the colossally large, to blend the waywardness of atoms with the stability of stars.

The publication of Professor Eddington's very notable treatise, that aims at penetrating to the secrets of the inside of a star as the most promising way

of interpreting the phenomena of the exterior, makes one realize anew what enormous advances have been made in the past thirty years in our knowledge not only of astronomical phenomena, but also of physical processes. §

His work originated, as he has told us, in an attempt in 1916 to fathom the problem of the variation in the amount of light emitted by certain variable stars, known as Cepheids from the fact that *δ Cephei* is a notable example of the class. These stars have a peculiar interest, inasmuch as, though spectroscopic observations seemed to prove them to be binary stars with orbital periods equal to the periods of their light variation, the phases of maxima and minima of luminosity do not conform in any simple manner with the orbital motions of binary systems. On the other hand, there are strong indications that regular pulsations of a single body, which alternately assumes a large radius and a small radius, pulsating rhythmically about a mean radius, would produce consistency between the light curve and the orbital motions disclosed by the spectroscope. The problem, then, of Cepheid variation was to find some cause that would be capable of maintaining the mechanical energy of regular pulsations. Enormous motions of distensions and contractions have to be kept going with complete regularity, with periods ranging from about forty days to a fraction of a day, with a semi-amplitude of something like 10^6 km. Energy must be swiftly available against dissipative forces. The maximum luminosity occurs simultaneously with the greatest velocity of approach towards the spectroscopic observer, and the minimum luminosity simultaneously with the greatest velocity of recession. This means that the star is bright when it is becoming distended, and faint when it is contracting; the maximum luminosity comes when the star's radius has expanded to its mean value, and the minimum luminosity when it has contracted to its mean value. If the energy is to be supplied from within, it must be applied at the right phase; otherwise the pulsations must either decay or else increase. It seems that there must be some countervailing tendency to prevent increase beyond stability. The clue that Professor Eddington held to be most likely to be helpful consisted in regarding the material of the star as the working substance of a thermodynamic engine, radiation from within being the transporter of heat, radiation pressure providing motive power, and opacity being the controlling influence. Obviously a knowledge of the conditions of the steady state of non-pulsating equilibrium was of the highest importance, and attention was diverted from the Cepheid variation to the larger problem of Radiative Equilibrium in the interior of a non-variable star.

The book summarises researches continued by him through ten years, and published from time to time from 1916 to 1925 in the form of more than a dozen papers of great interest and importance; and students who wish to study these papers will be well advised to read the summary given in Appendix II., pp. 397-8.

The problem is surveyed in the broad and lucid chapter with which the book begins. The question is: How is the continuous flow of radiant energy from the surface of a star maintained century after century without any but an exceedingly slow change of luminosity? Astrophysical investigations have shown that the spectra of stars differ very considerably from one another, but at the same time a given spectrum may be connected with stars of very different masses and radii. Fortunately, we can derive knowledge of masses from the study of binary stars; we can learn about mass and radius from eclipsing variable binary stars; we can find out the radius of a star when we have found its parallax and apparent magnitude and spectrum. We have measures of the effective temperature T_e of the sun's surface not only from study of the distribution of energy in its spectrum, but also from determinations of the total radiation emitted. In various ways like these we have been able to gain sufficient knowledge for statistical generalisations. In particular the information obtained from knowledge of the absolute magnitudes of stars —*i.e.* the magnitudes deduced from the apparent magnitudes when account is taken of the distances known from measured parallaxes, the absolute magnitude being defined as the magnitude a star would have if brought to a distance of ten parsecs, or ten times the distance of a star with parallax equal to one second of arc—gives us definite data concerning the luminosities of the stars

relative to that of the sun, whose absolute magnitude is +4·85; in this way we learn that a given spectrum may be connected with stars differing greatly in mass and radius, and differing still more widely in luminosity—"giants" with small mean density and "dwarfs" of relatively large mean density.

Up to the end of the nineteenth century the view that held sway in the explanations of the persistent outflow of energy from the sun and the stars was that suggested by Helmholtz in 1853, viz. that each such body utilised the potential energy of the distribution of its mass, and emitted a quantity of radiation equivalent to the loss of that energy in continual contraction. The sun or the star was held to be in convective equilibrium, on the ground that the energy set free within its body could not reach the radiating surface. quickly enough by conduction. There were tumultuous features observed on the sun's disc and at his limb sufficient to encourage the idea that convection was the process by which the energy was brought from within to the surface.

As early as 1870 Homer Lane had shown how to compare the pressure, density and temperature inside stars constituted on one and the same model (homologous stars). He had also shown that the history of the development of a star (from a nebulous state in which the density is gradually increasing by reason of contraction) involved first a rise in temperature so long as the contracting mass retained the properties of a perfect gas, and later the rise of temperature would be followed by a fall, as the condensing matter of the star departed more and more from the perfectly gaseous state. This work of Lane's proceeded on the assumption that the resistance offered by the heated gas to compression was the only agency opposing the collapse of the star upon its centre; this means that the pressure of the gas at any point in the star is sufficient to oppose the inward thrust of the gravitating matter; that is, is able to carry the whole weight of the layers above the point. This assumption provided for the mechanical equilibrium of the star. On the other hand, thermal equilibrium was assumed to be maintained by the automatic movement of masses of the gas within the star; any mass which was colder than its surroundings would be too heavy, and hence would fall inwards; and on the other hand a mass of gas that was hotter than its surroundings would be too light, and so would be driven towards the surface.

The work of Lane was amplified by August Ritter and by Kelvin in the 'seventies and 'eighties of last century, and by Emden in 1907, who treated the subject in an exhaustive manner, disclosing many of the irreconcilable mysteries, and publishing a number of tabular statements of the distribution of pressure, density and temperature in stars constituted on different models.

These early pioneers adopted convection as the mode of transport of heat inside a star, and their successors accepted that limitation on the ground that it was thought that the densities involved in the interior precluded the idea that heat could be carried by radiative processes. But that was in the days before radioactive processes were known to us, and before the properties of X-rays were discovered.

R. A. Sampson had in 1894 attempted a discussion of the internal structure of the sun, but his work was carried out in the years before the laws of radiation had been put on a firm basis by the experimental work of Paschen, Lummer, Pringsheim and Kurlbaum, and before the theoretical work of Rayleigh, Jeans, Planck, Wien and others in 1898-1906.

Schwarzschild in 1906 made a decisive advance in studying the possibilities of radiative equilibrium as of essential importance in an explanation of the phenomena of the sun's outer layers or atmosphere.

It was not till 1916 that Eddington, attracted, as has been said, by the mysteries of the Cepheid variables, was able to utilise the vast extensions of our knowledge of atomic processes and the properties of the short-wave radiation of X-rays, in order to discard the slow processes of convection as the mode of heat-transport inside stars and to adopt the swiftest possible mode of transport of energy—radiation. His book is full of indications of the intricacies of the problem attacked. His task has been to investigate the conditions in which mechanical and thermal equilibrium of the stars can be maintained through ages a million-fold more enduring than those deduced from the older theory; to reconcile the conditions with those accumulative

evidences of stellar evolution which have been disclosed by recent studies of their spectra; and to see that there shall be no transgression of the laws which physicists have found to govern the behaviour of X-rays and the disintegration of material atoms. He finds that the problem can be solved only if it be regarded as legitimate to appeal to atoms for a large measure of the energy which they contain by reason of their complex structure.

The simplicity of the initial mechanism involved in the mathematical analysis is very striking. After four instructive chapters on the Thermodynamics of Radiation, the Quantum Theory, Polytropic Gas Spheres, and Radiative Equilibrium, Eddington begins his sixth chapter by setting down the fundamental equations of the theory of the interior of a spherical non-rotating star.

$$\frac{dP}{dr} = -g\rho; \quad \text{and} \quad \frac{dp_R}{dr} = -\frac{k\rho H}{c},$$

H being the energy of radiation per c.c. per sec., of which $k\rho H\,dr$ is absorbed in a layer of thickness dr.

He takes the whole pressure P as made up not only of gas pressure p_G, but also of radiation pressure p_R; so that $P = p_G + p_R$; and accordingly

$$dp_R = -kH \,.\, dP/cg.$$

Here we have a notable departure from the old mode of attack, inasmuch as radiation pressure in the enclosed interior of a star is isotropic and analogous to hydrostatic pressure like p_G, and is accordingly regarded as capable of keeping the elasticity of gas to support the weight of the superincumbent columns of stellar material.

In a steady state the amount of radiation $4\pi r^2 H$ flowing per sec. across a sphere of radius r inside the star is the amount of energy L_R liberated per second within the sphere, probably from subatomic sources. Thus

$$\frac{H}{g} = \frac{1}{4\pi G} \cdot \frac{L_R}{M_R}.$$

The problem is to find out where and how this average rate of liberation L_R/M_R of energy per gram takes splace. Presumably it is greater near the hot and dense centre than at the "boundary" at r. It is legitimate to put $\frac{L_R}{M_R} = \eta \frac{L}{M}$, *i.e.* η times the surface value L/M, so that η increases from 1 at the boundary to some unknown greater value at the centre, and to leave this change of value to be adjusted after attention to a complexity of physical details. Meanwhile we have

$$dp_R = \frac{L}{4\pi cGM} \cdot \eta k\, dP.$$

Now, if we may put $\eta k = \text{constant} = k_0$, a simplification results, viz. that the ratio p_R/p_G is constant throughout the star. Eddington now introduces a constant such that $p_G = \beta P$, and $p_R = (1-\beta)P$.

The integration of the differential equation for p_R would introduce a constant, but it is of negligible amount, and we obtain the important equation

$$L = 4\pi cGM(1-\beta)/k_0,$$

applicable to some arbitrary "boundary," which may well be taken considerably within the real boundary, at a distance from it such that the main mass of the star is contained within it, and that the temperature there is still vastly higher than the effective surface temperature T_e of the star, though much lower than the central temperature.

If now we add the condition that the star's material is a perfect gas, we have

$$p_G = \frac{\mathfrak{R}}{\mu} \rho T,$$

and

$$p_R = \tfrac{1}{3} a T^4,$$

the latter being the pressure of isotropic radiation within an effectively enclosed space. On eliminating T, we obtain

$$P = K\rho^{\frac{4}{3}},$$

and so arrive at the equation

$$1-\beta = \cdot 00309\ (M/\odot)^2\mu^4\beta^4,$$

the solution of which gives us β in terms of only the mass of the star relative to the sun's mass $\odot$, and the mean molecular weight μ of the star's material, and enables us to find L from $L = 4\pi cGM(1-\beta)/k_0$. To this last equation, together with the quartic in β, may be added

$$\frac{T^3}{\rho} = \frac{3\mathfrak{R}}{a\mu}(1-\beta).$$

Now the relation $P = k\rho^4$ is a special case of the general relation $P = k\rho^\gamma$, utilised by Kelvin, Emden and others in developing the conditions of convective equilibrium from a knowledge of the ratio γ of the specific heats of gases. The general case serves for a family of distributions of pressure and temperature; and by taking different values of k and γ, a series of "polytropic distributions" can be brought under survey, and among them it should be possible to pick out one which approximates to that which carries conviction as to its suitability. Eddington's treatment has led to a particular case of polytropic distribution suitable for the conditions of radiative equilibrium. Fortunately his γ, which is decided by the heat-bearing properties of radiation, has the same numerical value as serves for the older convective equilibrium, and he is able to use the appropriate tabular statements of polytropic distributions published by Emden in his treatise on *Gaskugeln* (1907).

Eddington clears the way by discussing certain important minimal problems relating to central pressure and central temperature and mean temperature, and establishes confidence in the results of detailed analysis by frequent considerations of a general character. It is, in fact, an attractive feature of the book, that the author is continually testing the plausibility of his results by appeals to general considerations and criteria afforded by maximal or minimal values, where they are available.

When once the general idea has been found hopeful (viz. that the observed emission of radiation from the boundary of a star is approximately decided by the amount of radiation passing outwards from the central parts to the boundary), the complete solution is seen to depend on the adjustment of the disposable constants to fit the multitudinous physical properties of matter in the presence of radiation at temperatures and densities which are outside the limits of our terrestrial experience.

The outstanding difficulties consist in fixing the seat, the manner and the control of the liberation, within the star, of the requisite energy to provide the supply of radiation continually reaching us from the surface of a star. Eddington at once attacks the question of the seat of liberation by discussing the limiting case of liberation of energy from a point source at the centre of a star.

Chapter VII., on the "Mass-Luminosity Relation," begins with a number of examples showing how the formulæ derived are employed in calculations relating to specific stars. It goes on to the tracing of the theoretical curve connecting the calculated relation between absolute magnitude m and log Mass, deduced from the theory as previously developed. Then observational data m and M, relating to 37 different stars (selected by an independent arbiter, Hertzspring, to whom with Russell we owe the giant and dwarf theory of stellar evolution), are plotted for comparison with the theoretical curve, each star contributing a point on the diagram. The close clustering of the 37 star-points along the curve shows how closely the developed theory connects the mass and luminosity, for masses ranging from $\frac{1}{4}\odot$ to $25\odot$ and for luminosities ranging over 16 magnitudes, including all sorts of spectral types.

Contrary to expectation, the assumption of the condition of perfect gas seems to fit stars whose mean density is greater than that of water. Here there are no indications of a bifurcation into two curves, one relating to giant stars of low mean density, the other relating to dwarf stars of much greater mean density. It now becomes necessary (1) to hold the giant and dwarf theory of evolution as of doubtful import, and (2) to regard the internal material of a star as made up of atomic nuclei stripped of their "crinoline" appendages

of planetary electrons, so as to be capable of much closer packing. We must, moreover, scrutinise very closely the observational material on which the theory of giants and dwarfs rests, most especially in the gathering of assurance that the clear separation of two groups, giants and dwarfs, of stars of each of the spectral types G, K and M is not an effect of unconscious or possibly unavoidable selection of observational material. We must examine the evidence to be afforded by the extreme cases of what may be called leviathians and pigmies, it being borne in mind that the spectral characteristics of stellar luminosity still require to be systematically bound to the radiative properties of the inner material with which Eddington deals.

Then comes a chapter on "Variable Stars," containing much about the Cepheid variation. From it we gain a warning against admitting inadvertently conditions that would bring about either disruption or collapse of stellar structures.

Next a very searching chapter on the complex conditions involved in the treatment of the coefficient of opacity. It would appear that the relation for the coefficient should be of the form $k\alpha P/T^{\frac{7}{2}}$, which is consistent with Kramer's theory for the case of X-rays in terrestrial conditions.

But ionisation introduces in the stars conditions essentially different from those present in terrestrial experiments. For since absorption is mainly connected with the photoelectric effect, ionisation must necessarily reduce absorption. But even when account is taken of the diminution of the photo-electric effect due to ionisation, there remains a wide discrepancy between stellar and terrestrial values of k. [Attention may be called to errors in the last lines of page 225, where the values of P from Kramers' paper are given ten times too large. Errors and misprints are exceedingly rare in the book.] When one considers that $k \propto \lambda^3$, and that Planck's Law of Radiation, verified for optical frequencies, is used for extrapolation into regions of frequency (such as are to be found in X-rays) and of temperatures (such as are to be expected in stellar interiors), the wonder is that such discrepancies as are detected should be as small as they appear to be.

Next a Chapter X. on "Ionisation, Diffusion and Rotation." Ionisation is of importance in its bearing on the derivation of the effective value of the atomic weight μ to be assigned, not only in average value for stars in general, but also in the changes from the centre to the outer parts.

The effect of diffusion is considered in its effect on viscosity. Jean's work has shown that, when transfer of energy takes place by radiative processes, in which radiation is regarded as the equivalent of an outflow of mass, ordinary material viscosity is small compared with radiative viscosity. The sun is radiating at a rate which is equivalent to the outflow of 4×10^6 tons of matter per sec.; and this comes from the interior and is carried with the velocity of light through the surrounding strata. If, as in the case of the sun, the star is rotating, this must necessarily produce peculiarities in the circulation at the surface.

The effect of rotation is of obvious importance, especially in connection with the rate of liberation of energy in different parts of a star; and the discrepant opinions of Jeans and of Eddington and Milne on von Zeipel's theorem warn us of the need for care in the treatment.

Chapter XI. (on the "Source of Stellar Energy") opens up a subject still necessarily mysterious. Without a controlled subatomic source, the supply of energy is quite inadequate to maintain the radiation through the ages. Eddington concludes that no source is of any avail unless it liberates energy in the deep interior of a star. The alternatives appear to be (1) transmutation of the elements, either by disruption of heavy elements (radioactivity), or by combination of light elements into heavier, with partial disappearance of mass; and (2) the annulment of protons and electrons, with complete conversion of mass into radiation. The merits of the different choices are set forth, and an open verdict is returned.

Chapter XII. deals with the "Outside of a Star," a subject which brings us into touch with the observable phenomena of the stars, and though strictly speaking it is beyond the scope of his book, yet it is of prime importance in its

bearing on questions about the evolution of the heavenly bodies; and Eddington deals only with certain aspects that illustrate his main thesis.

The book closes with a chapter on "Diffuse Matter in Space," the subject of Professor Eddington's Bakerian Lecture in 1926.

If a brief summary of the results of Professor Eddington's work is asked for, it may be said that, in spite of certain still outstanding defects of complete numerical consistency between his theory and the observed phenomena of stars, he has shown with weighty argument that the observed luminosity of the stars, correlated with the effective temperature T_e of their external layers, must be attributed to the liberation of radiant energy in the deep interior, (1) dependent on subatomic processes, which in turn depend upon conditions related to the masses and radii of the stars; (2) controlled by opacity in a manner not yet fully understood; and (3) streaming through the stars, by reason of temperature gradients, with a gradual transformation, dependent on the opacity as affecting distribution of temperature, from the high frequencies of penetrating X-rays to the ten-thousandfold smaller frequencies of the finally emergent radiation.

His theory seems to be the only available one for extending the duration of the life of a star sufficiently beyond that indicated by Lord Kelvin's treatment before the days of X-rays and of the disintegration of atoms.

When attempt is made to study the comparative anatomy of the stars and to develop a theory of evolution of stars, then we have to realise that the material now at our disposal is not yet adequate to the task.

We can, at any rate, have but little doubt but that investigations like this of Professor Eddington, frankly facing the difficulties of finding a consistent presentation of various aspects of our knowledge, must serve to guide us along the right paths of research.

The whole account forms a very alluring study of an exceedingly complex subject, set forth in a manner that convinces the reader that he is being led along by a guide who is above all anxious to arrive at a true solution without overlooking any of the numerous pitfalls by the way.

In our picture of the Universe, we have to conclude that the life-histories of the stellar population overlap in every stage of life, with average durations measured in billions of years. We seem forced to admit that the lives of the individual stars involve a continuous conversion of matter into radiation. But that thought brings us up against beginnings and ends. What can we know of *them*? Kant wanted a cold beginning, Laplace a hot one, let come what would. Without some regenerative process whereby matter can somehow be reconstituted out of radiation, what hope is there for something like continuity, for which the mind yearns?

If $dM/dt = -Lc^2$, off goes the radiation to the "confines of the Universe," and as in R. L. Stevenson's story of the Bottle Imp, "the bottle goes out of the story."

And the Universe? Well, we can surely say that with or without continuity, it is full of wondrous miracles and beauty. "Cœli enarrant gloriam Dei, et opera manum ejus annuntiat firmamentum."

H. F. NEWALL.

Numerische Infinitesimalrechnung. By M. LINDOW. Pp. viii + 176, with 17 figures. Price, "kart" 15 M. 1928. (Dümmlers Verlag: Berlin.)

This book assumes a prior knowledge of the elements of the differential and integral calculus and goes on to prove and illustrate theorems about interpolation, numerical differentiation, numerical integration and the numerical treatment of differential equations, the latter with special reference to the astronomical problem of three bodies. Its contents thus overlap those of Whittaker and Robinson's *Calculus of Observations*, but differ in so far as Lindow includes a fuller treatment of differential equations; also in so far as Lindow omits determinants, roots of equations, least squares, smoothing, frequency curves and correlation.

As is customary in practical mathematics there is a convention that functions are to be understood to be single-valued, finite, continuous and differentiable unless the contrary is stated.

Lindow is careful not only to define his symbols, but often also to illustrate the definition. He shows a patience in exposition like that of one accustomed to teach students of middling ability.

The first three chapters include the interpolation formulae of Newton, Gauss, Stirling and Bessel, together with methods derived from them for numerical differentiation and integration. Tabulated numerical coefficients for use in the formulae occupy 19 pages at the end of the book. Ordinary and simultaneous differential equations (but not partial) are treated by several methods, including that of Runge-Kutta. All these are packed with the detail of precautions against error at every step. To one accustomed to the simplicity of the arithmetic of the deferred approach to the limit (Richardson and Gaunt, *Phil. Trans.*, A. 1927), this "concurrent" approach to the limit feels unnecessarily complicated. Yet, to quote a remark of an Astronomer, if there is a special risk like that of a comet going so close to Jupiter that a small error in calculation would make the computed comet pass on the side of Jupiter opposite to the actual comet, then accuracy at every step may be necessary. Lindow shows how to compute the perturbation of a planet's motion by Jupiter. He also treats the motion of two stars of equal mass near to each other and to a very light third body, all moving in one plane; in particular he discusses an orbit that just avoids a collision.

There is no attempt at a survey of modern work, for instance, Sheppard's operators, Frazer's lozenge diagram, Stömer's enormous computations and N. Kryloff's investigations of convergence all seem to escape notice. L. F. R.

Compound Interest and Annuity Tables. By F. C. & M. E. KENT. Pp. viii + 214. 20s. 1926. (M'Graw-Hill.)

This volume can be recommended to actuaries and others who use compound interest tables, and in particular to Americans.

It contains tables of the amounts of 1 and of 1 per annum and of the value of an annuity of 1, together with their reciprocals, to 10 places of decimals, up to 300 years for eight rates of interest between $\frac{1}{4}$ per cent. and $\frac{1}{2}$ per cent. inclusive, up to 200 years for four rates between $\frac{1}{4}$ per cent. and $1\frac{1}{4}$ per cent., and to 100 years for twenty-four rates between $1\frac{1}{2}$ per cent. and $10\frac{1}{2}$ per cent. These are followed by tables of other interest functions and of six-figure logarithms (seven-figure between 1·0 and 1·1). Finally is given the American Experience Table with commutation columns at $3\frac{1}{2}$ per cent.

A most pleasing feature is that the covers, while strong, are so flexible that the book can comfortably be held in one hand while being used. E. T. S.

The Theory of Functions of a Real Variable and the Theory of Fourier's Series. By E. W. HOBSON. Second Edition: Vol. 1 (1921), Vol. 2 (1926). (Cambridge University Press.)

SECOND NOTICE.

In a preliminary review a general survey has been taken of the first volume of this important work. The appearance of the second volume, containing, as was promised, portions indissolubly connected with what had been incorporated in the first volume, permits us now to undertake the discussion of the book in detail.

Before entering into the main subject, which commences only on p. 256 of the first volume, Professor Hobson presents us with an introductory section, to which he is then able to refer throughout his work. The titles of these introductory chapters are:

Chapter 1. Number.
Chapter 2. Descriptive Properties of Sets of Points.
Chapter 3. The Metric Properties of Sets of Points.
Chapter 4. Transfinite Numbers and Order Types.

In the Preface to the First Edition the author expresses his opinion that the subject here treated—the Theory of Sets of Points—"must be regarded as an integral part of the subject of his treatise." Yet he remarks that the Theory of Sets "has wide applications, not only in Pure Analysis, but also in Geometry," and, he might have added, in various other branches of pure and applied mathematics, and the tendency of the most recent times, more especially outside France, has been more and more to separate the Theory of

Sets definitely from the theories in which it receives its applications. In view of the fact, however, that Hobson uses terms and theorems which, though customary, are in form individual, it is a convenience to the reader to have at hand this preliminary sketch of the subject for subsequent reference. It does not pretend to completeness—we regret, by the way, the ignoring of Weierstrass's method in the discussion of the theory of the irrational number—but what has been given is a dignified and well-balanced survey of the field. The reader who is in search for information on the subject of Functions of a Real Variable will hardly dwell long on this part of the book, and the student whose interest lies chiefly in the subjects here treated will find the exposition clear and unequivocal, full of valuable references and affording room for individual research.

We propose here to call attention to one or two points which we should hope to see considered by the author in preparing a subsequent edition.

2. "The operation of counting," we are told in the opening sentence, "in which the integral numbers are employed, can be carried out by a mind to which discrete objects, which may be either physical or ideal, are presented, and which possesses certain fundamental notions which we proceed to specify."

These fundamental notions are unity, aggregate (or, as we call it, set), element, order, and correspondence. These, though certainly essential in the Theory of Functions, are hardly necessary for the mere purposes of counting, as exemplified by the performances of the late lamented horse Hans.

The psychological foundations of our knowledge, which Hobson is here skirting, are very difficult to deal with, and our author very properly deprecates (p. 3) going seriously into them. We may call our reader's attention to two recent books well worth reading in this connexion, Hugo Dingler's *Zusammenbruch der Wissenschaft* (Munich, 1926) and Fraenkel's ten lectures on the *Foundations of the Theory of Sets* (Leipzig, 1927).

3. These same fundamental notions, on which the author of a treatise on the Theory of Functions rightly insists, are those which underlie the use of what is called real numbers in mathematical analysis. The contrast of unity and plurality puzzled the early Greeks, so that they denied that unity was a number at all; the counterpart of this contrast is the essentially modern antithesis of element and set. In the book before us the superficial difficulty of abstracting from the external differences in objects and classifying them is touched upon, but there are deep-lying questions at the bottom of the so-called paradoxes of the Theory of Sets, and some of these are not even referred to. There is, for instance, the extension of the primary notion of set, corresponding to the Greek idea of number, as phenomenon (Erscheinungsform) or phantasm of plurality, to include a set consisting of a single object; and, again, there is the essential distinction in our minds between the same object regarded as an element (indivisible, either actually or potentially, that is whose one-ness is essential) and as set (potentially divisible, and only discovered to be actually indivisible at the conclusion of some argument, that is whose one-ness is accidental).

We should have liked in connexion with element to have had a clear exposition of the term range or fundamental region. The counterpart of the indivisibility of the element is the impossibility of extending the range; and it is the misapprehension of both these ideas that is responsible for most of the misconceptions that have been made by the philosophers in dealing with the foundations of the Theory of Sets.

4. An example of a so-called paradox, involving such a confusion of the two views of a single object, is that which deals with "a set which is one of its own elements," and an example has been devised of a supposed set illustrating this peculiarity of construction. It has been said: "The continued fraction

$$1+\frac{1}{1+}\,\frac{1}{1+}\cdots$$

is an object of thought which may be got by writing down in order the following four symbols:

$$a_1=1,\ a_2=+,\ a_3=1,\ a_4=1+\frac{1}{1+}\,\frac{1}{1+}\cdots,$$

the last being our continued fraction itself. Accordingly we may regard this as a set of which these are the four elements ; and we see that it is a set which is identical with its fourth element."

Formally this is deceptive ; the error involved is a confusion of terms. The continued fraction, quâ set, resolved in this manner, consists of its elements a_1, a_2, a_3, a_4, alone, without regard to their individual characteristics. As set it is the same as (1, 2, 3, 4) or as the set consisting of me, my father, my grandfather, and my great-grandfather. The actual things implied in this particular case by the symbols a_1, a_2, a_3 and a_4 are irrelevant ; when we regard them as elements, they have no parts and no properties. The set has on the other hand two properties, and two only, its elements and their order. Consequently it is only by a confusion of terms that the set can be identified with one of its elements ; it is as if Cardinal Wolsey could be confused with Faust because Henry Irving played both parts.

5. This same example may be looked at profitably from another point of view. In itself, it is a mere object of thought, and it is only by thinking that we can disintegrate it in our minds ; but this disintegration is not foreordained, we can do it in any convenient manner. For instance, if the particular mode referred to above (a_1, a_2, a_3, a_4) be legitimate, so is that into the following five elements :

$$b_1 = 1,\ b_2 = +,\ b_3 = \underline{1},\ b_4 = 1+,\ b_5 = \frac{1}{1+}\,\frac{1}{1+}\,\frac{1}{1+}\cdots,$$

no one of which could conceivably be confounded with the original continued fraction.

But here we are confronted with the fact that each of the ordered sets

$$(b_1, b_2, b_3, b_4, b_5)$$

and

$$(b_4, b_3, b_1, b_2, b_5)$$

represents equally the continued fraction. This illustrates another point requiring modification in the book before us : in dealing with order, Hobson says (p. 2) :

" Order may be regarded as an abstract conception, independent of a particular mode of ordering ; *for an aggregate to be an ordered one, it is necessary that, in some manner or other, each element be recognised as possessing a certain rank*, in virtue of which it is known as regards any two elements which may be chosen, which of them has the lower, and which the higher, rank " (the italicising is the reviewers').

6. The notion of order is one that still offers many difficulties, although much was done in the way of clearing it up by Georg Cantor's work on the subject, and, in particular, by his introduction of the notion of a set that is well-ordered, or, as Hobson prefers to term it, normally ordered. The reader will find on pp. 211 *seq.* of the present vol. i. a very carefully articulated account of Cantor's work. It would not only help to bring out the nature of Cantor's creative vision, but certainly enliven the presentation to add a reference to the serious misunderstandings made in the early days by Burali-Forti in two articles published in the *Rendiconti di Palermo*, vols. viii. and xi. 1894 and 1897. These have been much quoted since by writers who would appear not to have been at the pains to decipher the " Peanese " in which these articles themselves are written, since the errors in them have been entirely ignored and the statements made have been accepted as proven. Poincaré, for instance, in *Science et Méthode*, says that Burali-Forti had proved Cantor to be wrong ; (p. 201) and on p. 170 he relates in a very droll way a conversation on the subject with Hadamard in which he represents himself as asking Hadamard whether the reasoning of Burali-Forti did not seem irreproachable, while Hadamard, although maintaining the authority of Cantor, also misses the fact that that reasoning, being based on a wrong definition, is beside the point. The only objection Hadamard makes is that Burali-Forti had no right to speak of the set of ALL the transfinite ordinal numbers. " Pardon," said Poincaré, " he had the right, since he could always put

$$\Omega = T'(\mathrm{No}, \bar{e} >) ! "$$

This argument failing to bring conviction—and, as Poincaré remarks parenthetically, it would have been a pity if it had, since Hadamard was quite right—Poincaré was able to finish his chapter with the remark that he feared he did not speak Peanese with sufficient eloquence, or else, Peano's symbolism, the Pasigraphy, though quite efficient if we want to prove that 1 is a number, becomes impotent in face of an antinomy to be explained.

Hobson also refers several times to what he calls the antinomy of Burali-Forti, and on p. 245 he even gives the reference to the second of the papers above cited, without taking account of the blunders, so that the passage in question requires, to say the least, emendation. It is not correct to say that "this contradiction in the conception of the aggregate W of all order types of normally ordered aggregates was pointed out by Burali-Forti." A few lines further on Hobson says: "Burali-Forti inferred that ordinal numbers are not comparable with one another"; but this statement of Hobson's is misleading, as what Burali-Forti called "ordinal numbers" are not what we, following Cantor, call "ordinal numbers" at all.

In a letter to one of the present reviewers, then only a novice in the Mengenlehre, Georg Cantor himself pointed this out. "What Burali-Forti has produced" he wrote "is thoroughly foolish. If you go back to his articles in the *Circolo Matematico* you will remark that he has not even understood properly the concept of a well-ordered set" (9/iii/1907).

Cantor's original definition, to which that given by Hobson is equivalent, though less conversational, is as follows:

"By a well-ordered set is to be understood any well-defined set in which the elements are bound to one another in a definite preassigned succession according to which

"(i) there is a first element of the set, and

"(ii) not only is there to each individual element (provided it is not the last in the succession) a determinate next following, but also

"(iii) to each arbitrary finite or infinite set of the elements there belongs a determinate element which is the next after all of them in the succession (unless, of course, there is in the succession no element after them all)."

Burali-Forti in the first of his two memoirs * defines what he calls a well-defined set by the properties (i) and (ii) alone. Hence according to both Cantor and Burali-Forti, for instance, the succession

(I) $1, 3, 5, \ldots, 2n-1, 2n+1, \ldots$ *ad inf.*; $2, 4, 6, \ldots, 2m, 2m+2, \ldots$ *ad inf.*

(consisting of all the odd, followed by all the even, numbers, both in ascending order) is well-ordered. But if we reverse the order of the even numbers

(II) $1, 2, 3, \ldots, 2n-1, 2n-3, \ldots$ *ad inf.*; *de inf.* $\ldots 2m+2, 2m, \ldots 6, 4, 2,$

we get a set which is well-ordered in Burali-Forti's sense, but not in Cantor's.

In Burali-Forti's second memoir he would appear to have perceived that there was some discrepancy, but he by no means corrects his error; on the contrary, he accentuates it. He reiterates his imperfect definition of a well-ordered set, but he introduces a new term "perfectly ordered set," defined by the two conditions (i) and (ii) and the following additional one:

"(c.) Whatever be the element x of the set u, we either have:

"(c. I) x has no element immediately preceding it; or

"(c. II) there exist an element y, which precedes x, and has no element immediately preceding it, and is such that the element of u which at the same time follow y and precede x form a finite set." †

What he calls the ordinal numbers are the order types of these so-called perfectly ordered sets, but these are not the ordinal numbers of Cantor, which are the order types of well-ordered sets, defined as Cantor defined them. Indeed Burali-Forti has not, by the addition of his condition (c) arrived at a well-ordered set in Cantor's sense.

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* In which the definition of order (p. 170) contains a lapsus tacitly corrected in the second memoir.

† "(c.) Qualunque sia l'elemento x di u si ha che: o x non ha l'immediatemente precedente; o esiste un precedente y di x, non avente l'immediatemente precedente, tale che gli u che sono, ad un tempo, sequenti di y e precedenti di x, formano una classe finita" (*loc. cit.* p. 157).

It is true, as Burali-Forti himself observed, that the set (I) above cited is, and the set (II) is not, what he calls perfectly ordered, but he is in error when he asserts * that both these sets are well-ordered in Cantor's sense.

Again it would be fallacious to interpret Burali-Forti's assertion that "a perfectly ordered set is in consequence well-ordered" † in Cantor's sense. Indeed it is easy to construct sets which are perfectly ordered in Burali-Forti's sense without being well-ordered in the usual sense. Cantor's definition, as pointed out by Jourdain,‡ is equivalent to the following: "A set M is well-ordered if, and only if, it contains no part whose elements follow one another like the positive integers in descending order of magnitude," while Burali-Forti's condition (c) only has to do with what may be called the immediate neighbourhood of each element, and cannot affect the order of a subset each of whose elements is, in the original set, separated from the next by an infinite number of elements of the original set not belonging to the subset.

For instance, we may arrange in a column all the odd numbers, beginning with 1, and continuing with the remaining odd numbers in *descending* order of magnitude; and then we may fill in rows, beginning with each of these odd numbers, and continuing with the even numbers got from it by multiplying it in succession by the powers of 2 in *ascending* order. We thus get all the integers arranged in rows as follows:

$$1, 2, 4, 8, \dots, 2^n, \dots \textit{ad inf.};$$
$$\dots\dots\dots\dots\dots\dots\dots\dots$$
$$\dots\dots\dots\dots\dots\dots\dots\dots$$
$$(2m-1),\ 2(2m-1),\ 4(2m-1),\ 8(2m-1),\ \dots \textit{ad inf.};$$
$$\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots$$
$$\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots$$
$$7, 14, 28, 56, \dots 2^n \cdot 7, \dots \textit{ad inf.};$$
$$5, 10, 20, 40, \dots 2^n \cdot 5, \dots \textit{ad inf.};$$
$$3, 6, 12, 24, \dots 2^n \cdot 3, \dots \textit{ad inf.}$$

Reading the integers off row by row, we get an ordered, but not well-ordered, set satisfying all Burali-Forti's requirements for a perfectly ordered set. Indeed

(i) the first element is 1;

(ii) the next element to any integer is the number got by doubling that integer; while

(c. I) each odd number has no element immediately preceding it; and

(c. II) each even number $2^n(2m-1)=x$ determines uniquely the odd number $y=2m-1$, which precedes x and has no element immediately preceding it, and is such that the integers which at the same time follow y and precede x are $(n-1)$ in number, and therefore form a finite set.

It is easy to construct any number of examples of the same kind, so that no doubt can be left of the truth of Cantor's assertion that the discussion raised by Burali-Forti is entirely beside the point. What is curious is that people should have for so long taken the memoir quoted seriously at all. The only explanation would seem to be that, like Poincaré, they were not sufficiently proficient in Peanese. On the other hand, the superficial reader, even after having convinced himself of the justice of Cantor's criticism, might retain some doubts as to whether after all Burali-Forti's arguments were not applicable to the ordinal numbers of Cantor, provided the erroneous definitions of Burali-Forti were put right, for he would find on p. 161 of the memoir

* *Rend. di Pal.* xi. p. 157. "Il sig. G. Cantor chiama 'classe ben ordinata' quella classe ordinata che soddisfa alle condizioni (*a*), (*b*)" (*i.e.* (i), (ii), *supra*). "Avvi notevole differenza tra la classe ben ordinata e la classe perfettamante ordinata: sieno, p. es., $a_1, a_2, a_3, \dots b_1, b_2, b_3, \dots$ gli elementi di due classi numerabili; se consideriamo la classe ordinata $a_1, a_2, a_3, \dots b_3, b_2, b_1$, si vede facilmente che essa è classe ben ordinata ma non perfettamente ordinata, mentre $a_1, a_2, a_3, \dots b_1, b_2, b_3, \dots$ è classe perfettamente ordinata e, in consequenza, anche bene ordinata."

† *Loc. cit.*, see preceding footnote.

‡ Quoted by Hobson vol. i, p. 212.

quoted, in a footnote, the following remark: "Cantor says *ordinal number* in place of 'ordinal type of a well-ordered set.' . . . But this has no influence on our conclusions, for which it is sufficient to prove that our order types are precisely the order types of Cantor." *

That this remark is unjustifiable, *however interpreted*, seems evident, since, as has been demonstrated above, the sets with which Burali-Forti deals—his perfectly ordered sets—include sets which are not well-ordered in Cantor's sense.

Again, on p. 162 we find the following as Prop. 30: "If a is an ordinal number, then $a+1$ is greater than a," or, in Peanese, "$a\epsilon$ No. 3, $a+1>a$." A few lines further down we find the startling statement that "the Prop. 30 is not in general true if, according to Cantor, we use 'ordinal number' to mean the order type of a well-ordered set, as is immediately deducible from the example given at the end of § 3."

The example in question is that given above as (II); it is true that the addition of a single new element at the end of it will not alter its type, so that the new set will have the same type as the old. But the set is not a well-ordered set, and this fact is itself illuminating, both as showing why Cantor defined a well-ordered set as he did, and how misleading Burali-Forti's writing is. In short, Burali-Forti set out to prove that Georg Cantor had made a mistake, and by the use of a certain amount of mathematical hocus-pocus he succeeded in impressing a certain number of mathematicians, some of whom were by nature opposed to what Poincaré called "Cantorism," that there was something vitally wrong about Cantor's work. A glamour of pleasing mystery has gathered since about the so-called antinomies of which Burali-Forti's was the first. As a matter of fact, as in the case of so many conjuring tricks, all that is required is to look the thing straight in the face and use one's common sense. Without any Peanese, Burali-Forti's paradox is to be summed up as follows, and it may just as well be stated for well-ordered sets as for perfectly ordered sets, because the propositions used in Burali-Forti's proof can be paralleled in the theory of well-ordered sets, and the fundamental fallacy is in both cases the same. Take all possible well-ordered sets; these determine all possible ordinal numbers, and these, arranged as they can be, in ascending order of magnitude, satisfy the conditions defining a well-ordered set. The order type Ω of this set will accordingly be the greatest of all the ordinal numbers. Now, says Burali-Forti, take any set which is well-ordered and has the type Ω, and place after it a new element. We get a new set, which is still well-ordered, and contains the former set so that its order type is, by the fundamental properties of numbers, greater than Ω and is in fact, by rule, $\Omega+1$. But this is absurd, since Ω is the greatest ordinal.

Of course it is absurd. It is as absurd as to say "Take a cat that is a dog" or "Take an even prime number greater than 3." When you have taken in your mind a set of type Ω, you have taken everything, nothing remains to give a new element,† and the whole of the reasoning is mere confusion.

When Hadamard said that Burali-Forti had no right to speak of the set of ALL the transfinite ordinal numbers he was merely trying to beat off, somehow, anyhow, a waspish difficulty that offended him; and when Poincaré sneered at Peanese and said that Hadamard was quite right, he was only pooh-poohing what he considered a trivial study for a serious mathematician. Both were right and both were wrong. A serious mathematician has nothing to do with the set of every conceivable thing, but the philosophers have a perfect right to do so, and the followers of Peano, who presumably love their peculiar tongue as much as we do Saxon-English, may put down

$$\Omega = T' \text{ (No, } \overline{\epsilon} >)$$

* Il sig. Cantor dice veramente numero ordinale al posto di "tipo d'ordine di una classe ben ordinata." . . . Del resto ciò ha nessuna influenza sulle nostre conclusioni, per le quali basta sia provato che i nostri T sono precisamente i tipi d'ordine del sig. Cantor." "Scrivendo T al posto di 'tipo d'ordine'" (p. 159).

† It might be contended that we do not need to take a new element; it would suffice to take the first of our numbers and place it after the last, viz. *after* Ω. But the fallacy is really the same as the above, *there is no place after* Ω. Indeed the term "after Ω" has not been and cannot be defined, it is equivalent to "greater than the greatest."

if they please. But even a philosopher who has naturalised himself Peanese has no right to ask us to *take* what is *not to be taken* in any sense of the term, logical or otherwise; and no writer on the Theory of Functions of a Real Variable ought to leave his readers to suppose that Burali-Forti or anyone else, in Peanese or any other strange tongue, has proved the foundations of Cantor's theory to be unsound. W. H. and G. C. YOUNG.

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512. Howell, James, to his father: Baron Altham's son . . . "is full of excellent solid knowledge, as the mathematics, the law, and other material studies."—March 19, 1622.

513. "A great part of each number [of the *Analyst*] is unfortunately devoted to problems—the lowest form of mathematics—and the space available for more valuable matter is thus considerably diminished." . . .

. . . "No mathematical journal, if it is to contain anything of real value, can be elementary. Mathematics is an old science, and the really elementary parts of it must be acquired from text-books and by means of the examples which the student works out for himself as exercises. Elementary mathematics is a subject for the school-room, but is unsuitable for a journal, and such a publication as Mr. Martin's (the *Mathematical Magazine*), if it continues elementary, is educational rather than scientific. In no instance, I believe, has it been found possible to restrict a mathematical journal really to the elements of the subject alone, though of course elementary articles, and articles which are of interest to junior readers, form a considerable portion of each number. . . . Mathematical investigations that are really valuable can never be made elementary, and the questions that can be treated by elementary mathematics are too trivial to deserve recognition in a scientific journal."—J. W. L. Glaisher, *Nature*, Dec. 28, 1882, vol. 27, p. 195.

514. Sophie Kowalewsky, after intellectual achievements which have placed her among the most distinguished of her sex, pathetically wrote: "Why can no one love me? I could give more than most women, and yet the most insignificant women are loved and I am not."—Havelock Ellis, *Studies of the Psychology of Sex*, vol. vi. p. 141, Philadelphia 1913 [per F. Robbins].

515. We have three principal objects in the study of truth: one to discover it; another to demonstrate it; a third to discriminate it from the false when it is examined.—Pascal.

516. Lagrange uses *force* as often and variously as Newton used *vis*. *Force d'inertie*, *force vive* (what is *force morte*?), *force d'accusation*, *force motrix*, etc. Then there is Hooke's over-rated law: *Ut tensio sic vis*, where *vis* = extension if *tensio* = tension. [Letter from Sir G. Greenhill.]

517. Whittaker's *Dynamics* has no figure in it. Lagrange boasts that there was no figure in his *Mécanique*. Koenig deplores the same tendency. *Toujours Cours d'Analyse pas de géométrie; il nous faut un Chasles.*—[Letter from Sir G. Greenhill.]

518. Then there was Dodgson, with his clean, clear-cut, white face, tall hat and meticulous clerical uniform, the very model of a precise and starchy don, but already known to the whole world as the "Lewis Carroll" who wrote the two best children's books ever written. I think he taught only Euclid and a simple algebra to Pass-men, but he may have taught higher mathematics too, if anyone cared to learn them.—Nevinson, *Changes and Chances*, p. 41.

519. Theodore Martin tells us that Lyndhurst [Senior Wrangler, 1794] in his later years tested his reasoning powers by working out problems in the higher mathematics.

LONDON BRANCH.

THE Presidential Address will be delivered by Mr. W. C. Fletcher, M.A., at 3 p.m. on March 17th, at Bedford College.

Subject: The Regular and Semi-Regular Solids.

At the Annual Meeting, held on January 28th, the following officers were elected for the year 1928:

President—Alfred Lodge.
Chairman—W. C. Fletcher.
Vice-Chairmen—Miss M. J. Griffith; J. Katz.
Hon. Treasurer—W. M. Roberts, 22 Westmount Road, S.E. 9.
Hon. Secretaries—Miss L. A. Zelensky, Haberdashers' Aske's Girls' School, Acton, W. 3; F. C. Boon, Dulwich College, S.E. 21.
Committee—E. J. Atkinson; Miss E. G. Crowe; C. T. Daltry; F. G. Hall; S. Inman; Miss R. H. King; Miss M. G. Magrath; Miss M. K. Mitchener; Miss M. J. Parker; W. E. Paterson; Dr. Sheppard; Miss O. M. Stanton; H. V. Styler; Dr. Jessie White; Miss D. Yonge.

The Committee's Report recorded that the number of members for the year 1927 was 254, of whom 160 were full members and 94 associates. The usual six meetings had been held through the spring and autumn terms, all at Bedford College, and the attendance had been growing, twice in the autumn term threatening to overflow the seating capacity of the room.

During the year the Committee decided that a collection of text-books, accessible for consultation, might help members in search of books suitable for use in their classes, and that its usefulness would be greater if a catalogue were made containing such brief information of method and contents as would guide them in their search.

A bookcase to contain it has been bought, and by the kindness of the authorities of Bedford College housed in the room used for meetings, where the collection can be consulted on the afternoons of meetings or at other times (*e.g.* in vacations) by arrangement with one of the secretaries of the branch.

At present the collection consists of algebra text-books, with a catalogue prepared by a sub-committee, and some bound volumes of the *Mathematical Gazette*.

A similar collection of geometry books, with catalogue, is now being prepared, and the sub-committee will be glad to receive through F. C. Boon criticisms or appreciations of any text-books that members may be interested in.

THE MATHEMATICAL ASSOCIATION: SYDNEY BRANCH.

REPORT FOR THE YEAR 1927.

THE Sydney Branch has now 23 members of the parent Association, and 76 associates. Many of these members reside outside the metropolitan area, and so cannot in general attend meetings held in Sydney. The distribution of the *Gazette* continues to be the most important activity of the Association.

There have been two meetings held during the year; at the first Mr. Lyons, who had recently returned after spending a year in Cambridge, gave an interesting address on "Some Cambridge Geometers." The work of Dr. Baker and his associates was outlined, and some of the interesting results obtained were indicated.

The Annual Meeting was held during very inclement weather, but was well attended. A discussion took place on the mathematics papers set at the recent schools examinations. Mr. F. A. Booth addressed the meeting on "The Teaching of Mechanics."

This Association has suffered the loss by death of one of its oldest members in the person of J. D. St. Clair Maclardy, M.A., a distinguished graduate of the University of Sydney, a well-known and faithful servant of the Education Department of this State: and who was successively lecturer in the Teachers' College, for a short period its Principal, and finally Chief Examiner.

ERRATA.

Vol. XIV, p. 16, l. 6, *for* a' *read* $a' \mid A \mid$.

p. 23, l. 14 up, *for* Guyaelf *read* myself.

THE PROBLEM.

(By courtesy of the Editor of the *Observer*.)

If right and wrong were only plus
And minus, 'twould be well for us;
If reckoning up the life's amount
Were but a matter of account;
If we were able to assess
Virtue in Mammon's balances,
And weigh out like a pound of tea
Courage and faith and charity;
If two and two were always four,
And never less, and never more,
In that strange sum the Master sets—
Then one could toil, without regrets,
To puzzle the solution out,
And value x beyond a doubt.

But no kind axioms control
The mathematics of the soul;
Where definitions grow and fade,
And postulates are patched and frayed,
And ratios get out of hand,
Like Alice's in Wonderland;
And curious relativities
Upset our decimals and π's;
And a's are b's, and odds are evens
(The very sixes may be sevens!)
And good and bad, and wrong and right
Are hard to recognise at sight—
How, in this quandary, can we
Expect to reach the Q.E.D.?

F. H.

Observer, 1927, Dec. 11.

ROBERT WILLIAM GENESE

MAY 8, 1848—JANUARY 22, 1928

THE LIBRARY.

160 CASTLE HILL, READING.

The Librarian reports gifts as follows:

From Mr. **F. J. Cock**:

S. RAMANUJAN Collected Papers - - - - - - - 1927

From Prof. **A. S. Eddington**:

C. HUYGENS Œuvres XIII i-ii, XIV, XV - - - 1916, 1920, 1925

Of the fifteen volumes of this beautiful edition that have appeared, we lack only five, namely 3, 5, 6, 7, 12. Can any members help us to complete the set?

From Col. **T. C. J. Elliot**, a description of his Dial Machine offprinted from the *J. of Scientific Instruments*.

From Mr. **W. J. Greenstreet**:

N. R. CAMPBELL Physics: the Elements - - - - - - - 1920

The philosophical foundations of the subject.

A. REYMOND History of the Sciences in Greco-Roman Antiquity - 1927

Translated from French by R. Gheury de Bray.

From Mr. **T. W. Hope** and from Miss **G. T. Thomson**, collections of back numbers of the *Gazette*.

From Sir **Thomas Muir**:

List of the Serial Publications in the Libraries of S. Africa (2) - - 1927

Compiled by A. C. G. Lloyd, Librarian of the S.A. Public Library. The list is less comprehensive than its title, for it does not include art or literature, languages or history; perhaps the compiler feels a sly reluctance to certify every one of the 3117 publications as 'scientific'!

From Mr. **C. Pendlebury**:

C. A. A. BRIOT Théorie Mécanique de la Chaleur - - - - 1869

From Miss **C. Stimson**:

E. A. ABBOTT Flatland (3: W. Garnett) - - - - - - - 1926

Except for the editor's introduction and the imprint, this is a facsimile of the second edition, with the author's name added to the title-page. Into the second edition, which was published in the same year as the first, 1884, were inserted a preface and some pages of new matter.

From Prof. **D'Arcy Thompson**:

J. M. F. WRIGHT Solutions of the Cambridge Problems, from 1800 to 1820 (2 vols.) - - - - - - - - - 1825

From Mr. **F. W. Westaway**, a memoir:

I. TODHUNTER *On the method of least squares* - - - - - 1866

This short paper, from the *Transactions of the Cambridge Philosophical Society*, vol. 11, illustrates some of the difficulties of bibliography. First available as separate publications, as they still are, the papers forming these transactions were at that time paged separately when issued, and although the wrapper gives the volume, it is impossible on internal evidence to make a true page reference. A more serious matter is the date. The date given above appears on the wrapper and on the last page of the memoir. Merri-

man's *List* says 1869, and the R.S. Author-Catalogue gives the same date, but the R.S. Subject-Catalogue says [1865] 1871. The date 1865 is that of reading at the Society, and appears on the first page and on the last but one of the offprint, where the original paper ends; the date 1866 on the last page belongs to a note added after reading and before publication. Vol. 11 of the *Transactions* was issued in three parts, and it is to Part 2 that the date 1869 refers, while 1871 is the date on the title page of the complete volume. Keynes gives no date, and refers the paper to Vol. ii; whether this is the number of the part and the number of the volume has dropped out, or whether ii is a misprint for 11, who shall say?

From the Brown University Library, in exchange for a duplicate volume:

C. WARGNY Historia de las Matemáticas - - - - - 1907

Actually, but not expressly, a translation into Spanish of W. W. R. Ball's *Short History*.

EUCLIDES.

THIS is the new title conferred in the current volume on the periodical started in 1924 as *Bijvoegsel van het Nieuw Tijdschrift voor Wiskunde*. The serial numbering of the volumes is not interrupted, and the title, like the *Copernicus* of forty years ago, is only picturesque and must not be interpreted narrowly; the subtitle, *Tijdschrift voor de didactiek der exacte vakken*, indicates that the contents should interest members of the M.A. who are able to read them.

THE HALF-YEARLY JOURNAL OF THE MYSORE UNIVERSITY.

WE welcome fresh evidence of the vitality of Indian university life in this new periodical.

A REQUEST.

ANY member who is at any time about to throw a copy of the *Schoolmasters' Year Book*, perhaps a few years out of date, into the waste-paper basket, would be doing a kindness if he would send it instead to the Editor or the Librarian.

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520. Shrewsbury School: we had two wranglers to teach us, but they never taught anyone. Their appearance in form was hailed with indecent joy. As one of the classical masters said it was like the "Cease Fire" on a field day, and the whole body of boys abandoned themselves at once to relaxation. In the lower forms far-sailing darts were seen floating through the air as at a spiritualistic séance; in the upper we discussed the steeplechase or did Greek verse. A boy who really knew any mathematics was regarded by ourselves and the masters as a kind of freak. There was no dealing with him. His mathematical marks got him into forms beyond his real knowledge—his knowledge of Greek. He upset the natural order of things. He was a perpetual Ugly Duckling that could not emit Iambics. So his lot was far from enviable, and happily I remember only two such cases.—Nevinson, *Changes and Chances*, p. 28.

521. Byron to Dallas, Jan. 31, 1808: (I have seen) of mathematics enough to give me the headache without clearing the part affected; of philosophy, astronomy and metaphysics, more than I can comprehend; and of common sense so little, that I mean to leave a Byronian prize at each of our Almae Matres for the first discovery,—though I fear rather that of the Longitude will precede it.

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AT THE UNIVERSITY PRESS, GLASGOW

BOOKS RECEIVED, CONTENTS OF JOURNALS, ETC.

March, 1928.

Publications de la Faculté des Sciences de l'Université Maseryk. (Přírodovědecká Fakulta, Kounicova 63, Brno.)

Borůvka, O. *Sur les correspondances analytiques entre deux plans projectifs.* Pp. 34. 1927. (No. 85.)

Horák, Z. *Sur une généralisation de la notion de variété.* Pp. 20. 1927. (No. 86.)

Zahradníček, J. *Determination by Dynamical Methods of the Elasticity Modulus of a Rod as Cantilever or with Loads.* Pp. 12. 1927. (No. 89.)

Bell, E. T. *Algebraic Arithmetic.* (American Mathematical Society. Colloquium. Vol. VII.) Pp. iv + 180. 2 $. 1927. (Am. Math. Soc. 501 West 116th St., New York.)

Cajori, F. *Briefe von C. H. Schumacher und W. Struve an F. R. Hassler.* Pp. 532-536. Reprint from *Sonderabdruck aus der Zeitschrift fur Instrumentenkunde.* 1927. Heft 11.

Davis, H. T. *A Survey of Methods for the Inversion of Integrals of the Volterra Type.* Indiana University Studies. Nos. 76, 77. Pp. 72. 1$. (University Bookstore, Bloomington, Indiana, U.S.A.)

Dodge, R. *Elementary Conditions of Human Variability.* Pp. 107. $1.50. 1927. (Columbia Univ. Press, New York.)

Donkin, A. E. *Test Papers in Algebra. With Answers and Points essential to Answers.* 2nd Edition. Pp. 113 + 31a. 2s. 6d. 1927. (Pitman.)

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Eisenhart, L. P. *Non-Riemannian Geometry.* Pp. viii + 184. $2.50. 1927. (American Math. Soc., Bowes & Bowes, Cambridge.)

Hardy, G. H., Aiyar, P. V. Seshu, and Wilson, B. M. *Collected Papers of Srinivasa Ramanujan.* Pp. xxxvi + 355. 30s. net. 1927. (Cam. Univ. Press.)

Harward, J. *The Epinomis of Plato.* Pp. 146. 5s. net. 1928. (Clarendon Press.)

Horn, J. *Gewöhnliche Differentialgleichungen.* 2nd Edition. (Göschen's Lehrbücherei.) I. *Gruppe. Reine Mathematik.* Band 10. Pp. viii + 194. M. 9. 1927. W. de Gruyter, Berlin, W. 10.)

Jeffreys, H. *Operational Methods in Mathematical Physics.* No. 23. Cambridge Tracts. Pp. vi + 101. 6s. 6d. 1927. (Cam. Univ. Press.)

Lacroix, A., and Ragot, C. L. *A Graphic Table combining Logarithms and Anti-Logarithms.* 6s. 1926. (Macmillan Co.)

Lainé, E. *Précis d'Analyse Mathématique.* Tome I. *Théorie des Fonctions d Variables réelles. Théorie des Fonctions Analytiques.* Pp. 232. 30 fr. 1927. Tome II. *Equations Differentielles. Géométrie Infinitésimale. Equations aux dérivées partielles.* Pp. 336. 40 fr. 1927.

Lindow, M. *Numerische Infinitesimalrechnung.* Pp. viii + 176. 15 M. 1928. (Dümmlers, Berlin, S.W. 68.)

Lloyd, A. C. G. *A List of the Serial Publications available in the Libraries and Scientific Institutions of the Union of South Africa.* Pp. 259. (Presented to the Library of the Math. Assoc. by Sir Thomas Muir, C.M.G., F.R.S.)

MacRobert, T. M. *Spherical Harmonics.* Pp. xii + 302. 15s. net. 1927. (Methuen.)

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Niewenglowski, B. *Questions d'Arithmétique.* Pp. 224. 12 fr. 1927.

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Whittaker, E. I. *A Treatise on the Analytical Dynamics of Particles and Rigid Bodies, with an Introduction to the Problem of the Three Bodies.* 3rd Edition. Pp. xiv+456. 25s. net. 1927. (Cam. Univ. Press.)

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Vol. 49: No. 3, July 1927.

Linear Ordinary Self-Adjoint Differential Equations of the Second Order. Pp. 309-320. A. P. WHEELER. *The Singularities of a Function defined by a Dirichlet Series.* Pp. 321-328. D. V. WIDDER. *On Entire Function Interpolation.* Pp. 329-342. I. M. SHEFFER. *Optics in Space of Constant Non-Vanishing.* Pp. 343-354. J. PIERPONT. *On an Imprimitive Group of Order 5184.* Pp. 355-366. J. R. MUSSELMAN. *Rational Involutorial Transformations in S_4 which leave invariant ∞^4 Quadric Varieties.* Pp. 367-382. H. C. SHAUB. *A Three-dimensional Quartic Variety in Four-Space.* Pp. 383-388. B. C. WONG. *Compound Singularities of the Rational Plane Quartic Curve.* Pp. 389-400. J. H. NEELLEY. *Pencils of Conics in the Galois Fields of Order 2^n.* Pp. 400-406. A. D. CAMPBELL. *On Generalised Lacunae.* Pp. 407-418. J. J. GERGEN. *Properties of certain Aggregate Functions.* Pp. 419-432. L. S. HILL. *The Theory of Group-Reduced Distributions.* Pp. 432-455. J. H. REDFIELD. *Close of the Tangential Process on the Rational Plane Cubic.* Pp. 456-461. F. E. ALLEN.

Vol. 49: No. 4, Oct. 1927.

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American Mathematical Monthly. (Math. Assoc. of America, Menasha.)

Vol. 34: No. 9, Nov. 1927.

The Classification of Quadrics in Euclidean N-Space by means of Covariants. Pp. 453-467. P. FRANKLIN. *The Theorems of Ceva and Menelaus and their Extension.* Pp. 468-472. P. WERNWICKE. *Remarks on the Probable Error of a Mean.* Pp. 472-476. C. C. CRAIG. *An Elementary Example of a Continuous Non-Differentiable Function.* Pp. 476-480. F. W. PERKINS. *Line Values of Powers of Trigonometrical Functions and their use in constructing Curves.* Pp. 481-484. H. A. BENDER. *An Example in Probability.* Pp. 484-486. R. E. MORRIS.

Vol. 34: No. 10, Dec. 1927.

Certain Mathematical Questions suggested by the True-False Test. Pp. 503-515. H. M. WALKER. *On a Problem arising out of the Theory of a certain Game.* Pp. 516-520. J. V. USPENSKY. *Tschirnhaus Transformations on certain Rational Cubics.* Pp. 521-524. R. GARVER. *Benjamin Peirce's Linear Associative Algebra and C. S. Peirce.* Pp. 525-527. R. C. ARCHIBALD. *A Generalisation of the Strophoid.* Pp. 527-529. F. H. HODGE. *On the Origin and Development of the Idea of "Per Cent."* Pp. 530-531. D. DATTA.

Annals of Mathematics. (Princeton University Press, N.J.)

Ser. 2. Vol. 28: No. 4, Sept. 1927.

Generalized Derivatives. Pp. 419-432. A. J. MARIA. *A Cremona Group Isomorphic with the Group of Twenty-seven Lines on a Cubic Surface.* Pp. 433-450. H. L. BLACK. *Transformations of the Krummer Criteria in connection with Fermat's Last Theorem* (second paper). Pp. 451-458. H. S. VANDIVER. *Theorems on Deducibility* (second paper). Pp. 459-471. C. H. LANGFORD. *On Some New Types of Non-Differentiable Functions.* Pp. 472-476. A. N. SINGH. *On the Problem of Coloring Maps in Four Colors, II.* Pp. 477-492. C. N. REYNOLDS, Jr. *Division Algebras of Order Sixteen.* Pp. 493-500. R. GARVER. *Note on a Theorem concerning Continuous Curves.* Pp. 501-502. W. L. AYRES. *Development of Functions in a System of Approximately Orthogonal Functions.* Pp. 503-514. W. D. CAIRNS. *Concerning the Summability of Double Series of a certain Type.* Pp. 515-533. G. M. MERRIMAN. *Double Elliptic Geometry in Terms of Point, Order and Congruence.* Pp. 515-548. D. H. FLANDERS. *The Replacement Theorem and related Questions in the Projective Geometry of Paths.* Pp. 549-561. T. Y. THOMAS. *On Types of Knotted Curves.* Pp. 562-586. J. W. ALEXANDER and G. B. BRIGGS. *A Problem in Minima.* Pp. 587-592. D. JACKSON. *On the Development of Arbitrary*

Functions in Series of Hermite's and Laguerre's Polynomials. Pp. 593-619. J. V. USPENSKY. *Parallelism and Transversality in a Sub-space of a General (Finsler) Space.* Pp. 620-628. J. H. TAYLOR. *On the Form of the Differential Equations of a System of Paths.* Pp. 629-630. A. CHURCH. *Differential Invariants of Relative Quadratic Differential Forms.* Pp. 631-688. T. Y. THOMAS and A. D. MICHAL.

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Development in Hermite polynomials. Pp. 1-13. M. H. STONE. *A generalization of the calculus of finite differences to include the differential calculus.* Pp. 14-16. J. P. BALLANTINE. *Relations satisfied by coefficients of periodic solutions.* Pp. 17-20. W. J. TRJITZINSKY. *An integral equation with an associated integral condition.* Pp. 21-37. L. GUGGENBÜHL. *Partition polynomials.* Pp. 38-46. E. T. BELL. *A new formulation for general algebra.* Pp. 47-60. J. W. YOUNG. *A determination of the groups of order p^5.* Pp. 61-72. H. A. BENDER. *Representation of functions determined by their initial values.* Pp. 73-78. W. J. TRJITZINSKY. *An integration method of summing series.* Pp. 79-87. G. JAMES. *On groups of order p^m, p being an odd prime, which contain an abelian sub-group of order p^{m-1}.* Pp. 88-94. H. A. BENDER. *Osculating derivative of a ruled surface.* Pp. 95-105. D. SUN. *Chains of congruences for the numerators and denominators of the Bernoulli numbers.* Pp. 106-112. J. L. BELL.

Bollettino della Unione Matematica Italiana. (Zanichelli, Bologna.)

Vol. 6 : No. 5, Dec. 1927.

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Bulletin of the American Mathematical Society. (Bowes & Bowes, Cambridge.)

Vol. 33 : No. 6, Nov.-Dec. 1927.

An Analysis of some General Propositions. Pp. 666-673. C. H. LANGFORD. *On the Polynomial Convergents of Power Series.* Pp. 673-676. W. M. WHYBURN. *The Binomial Quartic as a Normal Form.* Pp. 677-680. R. GARVER. *The Probability Law for the Intensity of a Trial Period with Data subject to the Gaussian Law.* Pp. 681-684. E. L. DODD. *Concerning Regular and Connected Point Sets.* Pp. 685-689. G. T. WHYBURN. *Note on the Fourier Development of Continuous Functions.* Pp. 689-692. A. H. COPELAND. *Zeros of a Function and of its Derivative.* Pp. 693-695. W. J. TRJITZINSKY. *Linear Inequalities in General Analysis.* Pp. 695-700. L. L. DINES. *Substitutions which transform a Regular Group into its Conjoint.* Pp. 701-706. G. A. MILLER. *The Functional Equation defining Diophantine Automorphisms.* Pp. 707-712. L. WEISNER. *All Positive Integers are Sums of Values of a Quadratic Function of x.* Pp. 713-720. L. E. DICKSON. *The Summability of Fourier Series.* Pp. 721-732. M. H. STONE. *On the Separation of the Plane by Irreducible Continua.* Pp. 733-744. W. A. WILSON. *On the Mapping of the Sextuples of the Symmetric Substitution Group G_6 in a Plane upon a Quadric.* Pp. 745-750. A. ENACH. *Congruences of Lines of Special Orientation relative to a Surface of Reference.* Pp. 750-759. M. C. FOSTER. *A new Table of the Zeros of the Bessel Functions $J_0(x)$ and $J_1(x)$ with corresponding values of $J_1(x)$ and $J_0(x)$.* Pp. 760-772. H. T. DAVIS and W. J. KIRKHAM.

L'Enseignement Mathématique. (Gauthier-Villars.)

Vol. 26 : Nos. 1, 2, 3, issued Oct. 1927.

Le cinquantière scientifique de P. Paul Appell. Pp. 5-11. A. BUHL. *Sur une propriété de la constante d'Euler.* Pp. 11-14. P. APPELL. *Sur la comparaison de certains procédés de sommation des séries divergentes.* Pp. 15-27. G. BOULIGAND. *Sur l'intégration des équations différentielles.* Pp. 28-52. E. LAINE. *Sur une nouvelle et importante généralisation de l'équation de Laplace.* Pp. 52-63. *Les fonctions additives d'ensemble, les fonctions de point à variation bornée et la généralisation de la notion d'espace à n-dimensions.* Pp. 63-77. R. C. YOUNG. *Sur les géodésiques de certains éléments linéaires.* Pp. 78-84. A. ROUSSEL. *Sur les déplacements isohodoïques.* Pp. 84-97. V. HLAVATY. *Sur le triangle des pieds des hauteurs.* Pp. 97—>. A. STREIT.

The Eugenics Review. (Macmillan.)

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Jahresbericht der Deutschen Mathematiker-Vereinigung. (Teubner, Leipzig.)

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Bericht über neuere Untersuchungen und Probleme aus der Theorie der algebraischen Zahlkörper. I. Pp. 233-311. H. HASSE. *G. Hessenberg.* Pp. 312-332. R. ROTHE. *H. von Mangoldt.* Pp. 332-348. K. KNOPP. *Mathematische Miszellen X. Ueber das Poissonsche Integral und fast stetige Funktionen.* Pp. 349-353. A. OSTROWSKI. *Wellengleichung und Telegraphengleichung,* Pp. 353-358. A. KORN. *Bemerkung zu einem Satze von Hartogs.* Pp. 359-361. H. WÄSCHE. *Bemerkungen zum Vitalischen Satze.* Pp. 361-364. E. JACOBSTHAL.

The Japanese Journal of Mathematics. (Imperial Academy House, Ueno Park, Tokyo.)

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Beiträge zur gruppentheorischen Begründung der Geometrie. IV : *Topologische Kennzeichung der räumlichen Elementargeometrie.* Pp. 103-119. W. SÜSS. *A Remark on Singular Points of an Analytic Function on its Convergence Circle.* Pp. 121-123. S. IZUMI. *On an Extension of the Theorem of Vivanti-Dienes.* Pp. 125-127. M. TSUJI. *On a Power Series which has only Algebraic Singularities on its Convergence Circle,* III. Pp. 129-139. M. TSUJI. *Eine Bemerkung über asymptotische Entwickelung von Functionen.* Pp. 141-145. S. IZUMI. *Ueber die Grössenordnung des absoluten Betrages von einen linearen inhomogenen Form,* IV. Pp. 147-167. S. FUKASAWA. *Ueber die Nullstellen der Integrale von gewöhnlichen linearen homogenen Differentialgleichungen.* Pp. 169-178. M. NAGUMO. *Ueber die Grundlagen der Wahrscheinlichkeitsrechnung.* Pp. 179-193. K. TAKETA. *On Steiner's Curvature-centroid.* Pp. 195-201. B. SU. *Zur relativen Differentialgeometrie,* III : *Ueber Relativ-Minimalflächen und Verbiegung.* Pp. 203-207. W. SÜSS. *Certain Double Systems of Ruled Surfaces in the Line Congruence.* Pp. 209-213. B. SU.

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The Theory of Envelopes of Plane Curves (cont.). Pp. 73-76. C. N. SRINIVASIENGAR. *The Biparabola.* Pp. 77-88. N. DURAIRAJAN. *Some Definite Integrals and related Continued Fractions.* Pp. 89-96. M. B. RAO and M. V. IYER. Notes. *The Contact Circle.* Pp. 49-51. M. B. RAO. *A Theorem relating to Orthocentric Tetrahedra and Octohedra.* Pp. 52-55. N. DURAIRAJAN. *On the order of* d (n), *the number of divisors of* n. Pp. 55-57. S. D. CHOWLA.

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The Journal of the London Mathematical Society. (Hodgson.)

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Journal of the Mathematical Association of Japan for Secondary Education. (Prof. Iwama, Tôkyô Higher School for Girls.)

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No. 6, Nov. 1927. No. 7, Dec. 1927.

Proceedings of the Physico-Mathematical Society of Japan. (Faculty of Science, Tôkyô Imperial University.)

Ser. 3. Vol. 9 : No. 7, June-July 1927.

Ser. 3. Vol. 9 : No. 8, Sept. 1927.

The Delta Functions. Pp. 145-151. K. HISASUE. *On a Proof of Picard's Theorem.* Pp. 152-155. T. SHIMIZU.

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PROGRAMME FOR 1929-30.

All meetings will be held at Bedford College, Regent's Park, N.W. 1, at 3 p.m.

1929.

Dec. 7th. Presidential Address : "Dimensional Theory as a School Subject." —Professor H. Levy.

1930.

Feb. 1st. Annual Meeting.

1. Annual Business.
2. Discussion of Members' Topics.

Mar. 1st. A talk on "Energy and some points in Statics."—Professor W. M. Roberts.

Mar. 22nd. Discussion "Are we satisfied with the present Syllabus in Mathematics for the General School Certificate ?"
Opener : C. L. Beaven.

Summer Meeting.—It is hoped to arrange a visit to a Gramophone Record Factory.

The Secretaries will be glad to receive :

1. Suggestions for discussion at the Annual Meeting.
2. Names of Members willing to take part in the Discussion on March 22nd.

THE MATHEMATICAL ASSOCIATION.

(An Association of Teachers and Students of Elementary Mathematics.)

"I hold every man a debtor to his profession; from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves, by way of amends, to be a help and an ornament thereunto."—BACON (Preface, *Maxims of Law*).

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Its purpose is to form a strong combination of all persons who are interested in promoting good methods of teaching mathematics. The Association has already been largely successful in this direction. It has become a recognised authority in its own department, and is continuing to exert an important influence on methods of examination.

The Annual Meeting of the Association is held in January. Other Meetings are held when desired. At these Meetings papers on elementary mathematics are read and discussed.

Branches of the Association have been formed in London, Bangor, Yorkshire, Bristol, Manchester, Cardiff, the Midlands (Birmingham), the North-Eastern District (Newcastle-upon-Tyne), New South Wales (Sydney), Queensland (Brisbane), and Victoria (Melbourne). Further information concerning these branches can be obtained from the Honorary Secretaries of the Association.

"**The Mathematical Gazette**" (published by Messrs. G. BELL & SONS, LTD.) is the organ of the Association. It is issued at least six times a year. The price per copy (to non-members) is usually 2s. 6d. each. The *Gazette* contains—

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